

Departure of Professor Hoyle

THE unhappy tale of Professor Fred Hoyle's resignation from Cambridge (see page 419) will leave a scar on British astronomy and will not conspicuously enhance the reputation of the University of Cambridge. It may be, as one Sunday newspaper has reported, that Professor Hoyle has described himself as "a difficult customer", but he is also a valuable one. In strictly scientific fields his work with W. H. Fowler on nucleogenesis and stellar evolution is more important than has yet been acknowledged. His more daring and speculative contributions to discussions such as the validity of steady state cosmology and unified theories of the universe have been provocative and stimulating and may yet—who knows?—turn out to be memorable. Quite apart from this, however, he has enlivened the scene with a prodigious amount of intellectual work—plays, popularizations and even science fiction. In the past few years, he has two important institutional achievements to his credit. The Institute of Theoretical Astronomy at Cambridge has been made into a centre of great repute, and many of its young men and women are already in charge of university departments elsewhere. Uncharacteristically, Professor Hoyle has also been, in his time, a committee man. His spell as chairman of the Space and Astronomy Board of the Science Research Council saw, first of all, the recognition that astronomy should be an essential part of British scientific research, the recognition of the wisdom of separating the post of Astronomer Royal from the scientific management of the Royal Greenwich Observatory and the development of a programme for cooperative instrumental research in the northern hemisphere that could, if it had been energetically pursued, by now have created a much more coherent framework for collaboration between Europe and North America than any that exists at present. Then Professor Hoyle has also been personally responsible for keeping close links with astronomers in the United States and a part at least of the benefit of that informal collaboration is probably the recruitment of Dr Margaret Burbidge as the new director of the Royal Greenwich Observatory.

The temptation to ask who is to blame should be suppressed, but there are a number of important questions which should not be ducked simply on the grounds that personalities are concerned. First of all, however, it should be openly acknowledged that there is no reason to suppose that Professor Hoyle's departure from Cambridge (due to be made final at the end of the current academic year) reflects unfavourably on Professor D. Lynden-Bell. All parties to the dispute agree that the issue is not the identity of the new professor of astronomy and head of the Cambridge Observatory but his scientific background. Professor Hoyle says that he was hoping for the appointment of a practical man, not a theoretician like himself. He may easily have misjudged the way in which, in modern astronomy, theoreticians can be a fertile source of instrumental innovation. But in any case, Professor Lynden-Bell deserves both encouragement in what will now be a more formidable task than he can originally have bargained for and sympathy for the invidious

position in which he now finds himself. For his sake, at least, it is important that the issue between Professor Hoyle and his colleagues at Cambridge should be openly discussed and fairly judged.

The scheme put forward a year ago for the merging of the Institute of Theoretical Astronomy and the Cambridge Observatories, indirectly the cause of Professor Hoyle's complaint, was sensible enough. Cambridge has won a high reputation in astronomy of all kinds, and there was every reason to expect that a merging of the chief centres of theoretical and observational work would have strengthened research and made teaching more effective. Professor Hoyle himself was a member of the committee which advocated this change, which implies that there is no disagreement of principle. (The fact that the Mullard Radio Astronomy Observatory was not included in the consortium is a conspicuous anomaly, but understandable.) What seems to have gone wrong is that the university failed to recognize that in merging the two institutions, the incumbent head of the Institute of Theoretical Astronomy would, if he were not a mouse, expect to have more than an equal say in the appointment of his partner, the head of the Cambridge Observatories. After all possible concessions have been made to the doctrine of democracy within the university, such an expectation is a necessary precondition for harmonious working.

The role of the Science Research Council in this whole affair is still obscure. Naturally enough, the council would have played no part in the appointment of a successor to Professor Redman, the retiring director of the observatory, but no doubt its influence would have been felt in the decision to merge the institute and the observatory, if only because the university must by then have recognized that only a rationalized organization would attract public support on a sufficient scale. All this of course is entirely proper. The puzzling part of the research council's involvement is that so little has been done to implement the recommendations of the working party on astronomy in the northern hemisphere or even to publish its report, now more than two years old.

The University of Cambridge is more directly implicated. There seems to be no doubt that all the proper democratic forms were observed. The appointment was properly advertised and, in due course, the selection committee met and deliberated. That it should not have done so sooner (see page 419) is something of a puzzle and no doubt some chronicler of university intrigue will one day explain precisely what went on. There is, however, a point when universities should be seen to act in a much more humane fashion. In Cambridge in the past few months, it should surely have been possible for somebody to have recognized that too methodical a grinding of the committee machinery might drive Professor Hoyle away. And even if Professor Hoyle may have seemed at times to be asking for more than his fair share of influence, surely there must have been some way in which more moderate colleagues could have worked a compromise. There are, after all, more of them than there are of him. In short,



the most unhappy feature of the Hoyle affair is that, rightly or wrongly, the university has allowed itself to seem inflexible. The hope now is that none of this will affect the quality of the work, quite exceptional by any standards, carried out at Cambridge.

Where Next with Apollo?

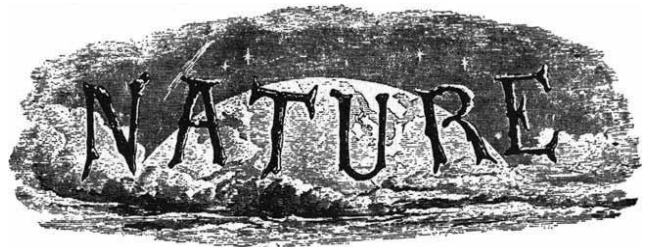
THE expedition of Apollo 16 to the Moon this week, technical hitches notwithstanding, has been yet another demonstration of the usefulness of these journeys. From the start of the Apollo programme, more than a decade ago, there has been good reason to think that the landing of men and equipment would bring a valuable harvest of information about the Moon, and now the expectations of the optimists have been for practical purposes confirmed. Each new expedition suggests new experiments that might be carried out, which is at once a sign that useful jobs remain to be done on the surface of the Moon and that there will be great frustration later in the year, as the time approaches for the next and last expedition, Apollo 17. To acknowledge this is not to say that the great cost of the Apollo programme has been justified, for a more balanced programme of research, some of it dependent on largely conventional techniques, would probably have made a still more remarkable contribution to the understanding of the Solar System. From the start, however, the Apollo programme has been openly regarded by its sponsors as an objective in its own right and only adventitiously a means of scientific research. The United States would have had an Apollo programme even if this had not by chance been a time when it is possible to ask pertinent questions about the origin of the Solar System and to make use of the Moon as a means of testing some striking hypotheses. If, for example, plate tectonics had not by now provided a framework for the understanding of geological events on the surface of the Earth, it would have been much more difficult to make sense of what the Apollo expeditions have found on the surface of the Moon. But is it a sensible use of resources that the next Apollo expedition to the Moon, at Christmas, should be the last?

The zeal with which the National Aeronautics and Space Administration pursued the planning for the Apollo programme a decade ago has now given way to enthusiasm for the space shuttle, a system of reusable rocket vehicles that will be used, in the late 1970s and the 1980s, for ferrying people and equipment to and from living spaces in orbit about the Earth, and which in due course may provide a cheaper method of exploring the Solar System than could ever have been provided by the Saturn rocket system. In due course, the space shuttle will also be a flexible tool, able to provide staging posts for all kinds of exploits, not necessarily including people. The trouble, of course, is that the shuttle—like the Concorde and its Russian rival—is way ahead of its time. In due course, it may turn out to be an economic blessing, but as things are, there is not enough business to make full use of it. And even by the late 1970s, it is unlikely that the United States Congress will be prepared to sanction such a large increase of the NASA budget that the shuttle system can be put usefully to work. Even if NASA's attempts to recruit European participants for the project

are more successful than has seemed likely for the past year, it is hard to think that the space shuttle will turn out to be more than a white elephant unless Congress is prepared to increase its support for NASA above the level of \$3,200 million a year which Dr James C. Fletcher has said he and his men will be able to rub along with.

The ending of the Apollo programme at this stage in the game is especially unfortunate. It may be that Apollo should never have been begun, but now that the milk has been spilt, is there not a good case for continuing with a modest programme of journeys to the Moon? Dr Fletcher was telling Congress only a few weeks ago that he and his colleagues would no doubt be embarrassed by their planned inactivity later in the decade, when it is most probable that the Soviet programme of planetary exploration will gather momentum. As it happens, NASA has the solution in its own hands, for there are spare Saturn V rockets in storage and enough is now known about the machinery involved in landing on the Moon to ensure that space capsules could be built in time. To be sure, there would be the extra cost of maintaining launch facilities, which would imply that extra Apollo journeys would be more expensive than in the past. Yet if NASA is serious in saying that it wishes to contribute to the understanding of the Solar System, it is feckless of it to embark on the shuttle instead.

100 Years Ago



THE CIVIL ENGINEERS' BANQUET

WE do not grudge our friends the Civil Engineers their annual felicitations, nor Mr. Gladstone his congenial moral reflections. It were hardly worth while to dissect after-dinner rhetoric, however full of fallacies. But those ever-watchful teachers of mankind, the daily press, have pounced upon the speeches delivered on Wednesday week, and have made them an occasion for propounding solemnly what was spoken hilariously; and this deserves looking to.

The *Times*, of course, armed at every point, does battle valiantly for decentralisation of science, because that notion seemed to find favour with the notabilities of the evening. Mr. Hawksley, president of the Civil Engineers' Institute, in toasting Her Majesty's Ministers, complimented them on the "performance of the negative duty of letting his profession alone," adding, with unconscious satire, that what the engineers had done "they had achieved, not through, but in spite of, all Governments." These two sentiments are quite intelligible and quite true; but the conclusion of the speech, which informs us that "the Civil Engineers of this country approached the Government with perfect reliance on its purity," conveyed a needless truism; to the pure all things are pure—to pure engineers even a British Government is pure, of course. But why dwell on so obvious a fact?

From Nature, 6, 1, May 2, 1872.

OLD WORLD

Cambridge Astronomy Without Sir Fred

SPECULATION about the motives for Sir Fred Hoyle's resignation from the Plumian chair of Astronomy and Experimental Philosophy at Cambridge, which was announced this week, is rife in astronomy circles in Britain.

The last straw which made Sir Fred tender his resignation according to reports from California where Sir Fred is this week attending a conference, was the decision of the university to appoint Professor Donald Lynden-Bell, currently of the Royal Greenwich Observatory and the University of Sussex to the chair of Astrophysics at Cambridge which falls vacant in October due to the retirement of Professor R. O. Redman, director of the Cambridge Observatories.

Professor Hoyle has made it clear that he has no personal objections to Professor Lynden-Bell but his protest is based on the fact that Lynden-Bell is a theoretical astronomer and Sir Fred thought that the university should have appointed an observational astronomer to the post.

Much confusion has been created by misinterpretations of the wording of the notice published last August in the *Cambridge University Reporter* asking for applications from suitable candidates for the chair of astronomy. At first glance it seems that the way in which the notice was written completely excludes theoretical astronomers from applying but a member of the board of electors to the chair of astrophysics pointed out this week that the notice was framed in such a way that this was not so. The announcement called for "an astrophysicist with an interest in fostering instrumental development". It seems that this wording was chosen after much discussion within the university and also after discussion with the Astronomy Policy and Grants Committee of the Science Research Council of which Sir Fred is the chairman.

The programme of research put forward by Professor Lynden-Bell fulfils these conditions, and according to one of the electors will ensure that the university and national policies on astronomy are dovetailed.

Electors of Cambridge University professorships hold office for four years and terms of office come to an end on the last day of September, four years after original appointment to the board. In the case of Professor Lynden-Bell's appointment the fact that two of the 1970-71 electors ended their terms of office in the time between the appearance of the notice of the vacancy and the appointment has led to charges that

the appointment was at best mishandled and at worst, underhand. Electors are not changed once they have started on their deliberation to choose a new professor. The present board of electors consists of Professor F. G. Smith, Professor Sir Martin Ryle, Professor Sir Harrie Massie, Professor Sir Nevill Mott, Professor Sir William Hodge, Professor A. B. Pippard, Sir Richard Woolley and Professor Sir Fred Hoyle. This board differs from that of 1970-71 in that on October 1, 1971, Professor F. G. Smith replaced Professor P. Hall, and Professor A. B. Pippard replaced Professor Sir Bernard Lovell. The charges which have been laid against the university are that the 1970-71 board started on the task of appointing Professor Redman's successor but that contrary to the regulations Professor Lovell and Professor Hall were replaced on the board before the deliberations were completed and the task was finished by the 1971-72 board.



Sir Fred Hoyle

It seems, however, that the 1970-71 board never actually met to discuss the new appointment, although efforts were made to get them together before September 30, 1971. These did not turn out to be successful as it proved impossible to find a day when all members of the board were together in Britain at the same time. Thus the appointment was made completely by the 1971-72 board which held its first meeting in October, 1971.

Although the appointment of Professor Lynden-Bell might have been the last straw there is no doubt that Sir Fred was far from happy with other recent developments at Cambridge. In particular the decision taken by the uni-

versity last November to merge the Institute of Theoretical Astronomy of which Sir Fred is the director, with the Cambridge Observatories in order to rationalize research in astronomy, has, it is rumoured, far from pleased Sir Fred Hoyle, signatory to the report which, even though he was a member, recommended the merger.

The Institute of Theoretical Astronomy was formed in 1966; it is widely believed that it was set up to keep Sir Fred Hoyle from joining the brain drain. The merger meant that Sir Fred would have lost his autonomy and it is now questionable whether the rich flow of summer visitors to the institute can continue in future years. The financing of the institute has been carried out on a three part basis with the Science Research Council providing 40 per cent of the cost which has amounted to £250,000 since 1966. The Nuffield Foundation has provided a similar amount while the University of Cambridge has put in £125,000 since the inception of the institute. The financing of the merged departments is still uncertain; the SRC report that they have been considering it since early this year but no decision has yet been made. What is clear, however, is that the Nuffield Foundation will not be asked to provide finance for the merged body.

Sir Fred Hoyle's resignation will be felt in British astronomy circles and if he leaves the country for another post he will leave gaps to be filled on the Science Research Council, the Astronomy Space and Radio Board, the Astronomy Policy and Grants Committee, of which he is currently chairman, and the Anglo-Australian Telescope Board, although a spokesman for the SRC pointed out this week that there was no compunction on him to resign if he went to work outside Britain.

With Sir Fred Hoyle's resignation British astronomy will have lost the advantage it gained last summer when Professor Margaret Burbidge was wooed back from the United States to be director of the Royal Greenwich Observatory. The consensus among astronomers is that Sir Fred's resignation will not lead to a massive drain of astronomers from Britain and that astronomers will still be keen to work either at the enlarged centre at Cambridge or at the Astronomy Centre at the University of Sussex where Dr Martin Rees has recently been appointed as professor in succession to Professor W. H. McCrea.

SCIENCE POLICY

Sir Brian Speaks out

THE achievements of science, whether medical or physical, are of no avail unless mankind can teach itself to make use of them. This was Sir Brian Flowers's theme when he delivered the annual Silvanus Thomson memorial lecture last week at a conference organized by the British Institute of Radiology.

Sir Brian, ebullient as ever, spoke about the support of scientific research with particular emphasis on collaboration between the various sciences. As has been his habit of late Sir Brian cocked a snoot at Lord Rothschild as well as criticizing his contribution to the notorious green paper.

As an apology for addressing the conference on a topic outside physics, Sir Brian said that "if that noble biologist, Lord Rothschild, can lecture the Royal Society of Arts about opportunity costs, whatever they may be, I can take heart from the old proverb 'What one fool can do another can'".

In a speech that can possibly be read as a statement of intent by the Science Research Council, Sir Brian emphasized that in future man can not observe and analyse the world "in Olympian detachment." Also, future technological developments "will be assessed not only for what they do for man but also for what they do to man".

Sir Brian reviewed the ecological crisis and emerged as the holder of a moderate view. He called for greater education to curb the growth rate of the population in underdeveloped countries and, as far as the depletion of the Earth's resources are concerned, he said that "society will increasingly devise means for their replacement through the development of synthetic substitutes and of new sources of power".

Sir Brian emphasized that the SRC's interests concern "the role of biology now, and in the future, and the inter-relations between biology and other branches of science". He added that, thanks to the achievements of the physical sciences and technology, man has made enormous strides in developing and adapting his environment to his own immediate needs. These achievements, according to Sir Brian, must, however, be looked upon as "perturbations of the ecosystem of which man is a part".

A nail was knocked into the coffin of the pure physical scientist when Sir Brian asked whether "we have now come to a time of pause in the affairs of physical science". This is not because of a belief such as existed at the turn of the century, that all is known—in fact, Sir Brian asserted that this is far from true, and that high energy physicists

and astronomers, in particular, are on the verge of important new discoveries. The reason, said Sir Brian, was that the scientist "is growing dissatisfied with present day motives and values, and is looking for a fresh, more biological approach in which man shall no longer be subservient to machine".

In yet another attack on Lord Rothschild's belief that research can be classified in neat packages, Sir Brian said that the nature of scientific research made it impossible to predict the outcome in advance. "Call that scientific roulette with Lord Rothschild if you will", Sir Brian added, but anyone who maintains that research is useless because its ultimate practical goal cannot be identified "would be a brave and foolish man". "The unexpected is the whole point of scientific research," said Sir Brian, and he amply demonstrated this by referring to the practical developments that have arisen from research into superconductivity.

The research councils are now faced with more difficult tasks than they have been faced with in the past, according to Sir Brian. No longer do they have only to decide on the relative priorities of well defined fields of research, such as astronomy or electrical engineering, but they also have to decide on priorities between projects that are sponsored by several research councils simultaneously — pollution research being one example.

Sir Brian used the platform to make yet another plea to the government to take a more moderate view of the reorganization of government research and development than has been suggested by Lord Rothschild. Sir Brian cited the Institute of Radiology's record in making use of new kinds of radiation in hospital medicine and he said that this transference of ideas from the research laboratory to the hospital is "what the organization of research must foster". Sir Brian concluded with a call to the formulators of the white paper on reorganizing government research and development not to forget the difficulties of predicting in advance the course of research.

SRC and Biology

THE Science Research Council said this week that its expenditure on biology was going up, in real terms, at 5 per cent a year—much faster than its support of nuclear physics, which is the biggest single field of research funded by the council. A spokesman for the council also said that Sir Brian Flowers's speech to the British Institute of Radiology did not "herald any immediate change in the SRC's priorities".

RESEARCH COUNCIL

ARC Foresees a Squeeze

THE annual report of the Agricultural Research Council published on Wednesday will be a disappointment to anyone who expected to read the council's reactions to Lord Rothschild's report published in the green paper of last November. In an apologetic tone the council regrets that as the annual report was written just after the green paper was published and before the council had submitted its views to the Secretary of State it "has not been possible to include any comment in this report". Rather lamely the council adds that nonetheless the "prolonged uncertainty" has "inevitably" inhibited long term planning and "undoubtedly" affected morale.

What the council does include in its report, however, is a detailed account of much of its research and a clear warning to university researchers that times are going to get harder. The council says that although the value of the research grants it gave rose by £116,699 to £586,393 in 1970-71, and the grants for purchasing items of equipment over £1,000 amounted to £60,961, "it is probable that the growth rate will have to be reduced in the future". More university departments are applying for grants the council says, partly due to the shortage of money elsewhere, and as a result the council will in future consider grants on only two occasions in the year, in April and July.

On a happier note, the council reports that an ICL 4/70 computer which the Flowers Report recommended that the ARC should have in 1966, was delivered to Rothamsted Experimental Station in the course of the year. Sixteen of the ARC's units are already on line to Rothamsted.

The research work described ranges from work on pre-slaughter stress in poultry to potato spraing disease and includes the news that a new synthetic pyrethroid has been synthesized at Rothamsted which has the advantage of acting as quickly as the natural pyrethrins and, when combined with a synergist, as efficiently as resmethrin—one of the synthetic pyrethroids originally discovered at Rothamsted (see *Nature*, 233, 441; 1971).

In the course of the year three ARC units were disbanded and no new ones established, but the council's expenditure still rose from £15,023,000 in 1970 to £16,932,000 in 1971, an increase of 13.3 per cent. The Institute of Animal Physiology at Babraham remained the largest spender of the ARC units at £815,871, but grants to the ARC's own units and institutes rose only £420,000 while its grants to other research institutes rose by £1,200,000.

WORLD HEALTH

Curbing Disease

UNLESS there is a vigorous intensification of research, the progress many countries are making in their fight against disease will be slowed down and may even be halted in some areas according to Dr M. Candau, director-general of the World Health Organization. In his introduction to the organization's twenty-third annual report (HMSO £1.50, published in English, Russian, French and Spanish) Dr Candau says that without significant discoveries the control of some diseases including certain vector borne parasitic diseases, cardiovascular disease and iron deficiency anaemia "is unlikely to make much progress in the next ten or even twenty years". But he adds "we know what needs to be done and what questions to ask, and we have the technical resources and the research skills to find the answers".

Treading the same middle road of cautious optimism, Dr Candau says that public opinion has become "unduly sensitive" during 1971 to the problems of cholera and the adverse effects of DDT. The problems "have been magnified out of all proportion to the real hazards". On DDT Dr Candau says that "the recognition of the fact that DDT might carry with it certain hazards should not be allowed to obscure its immense advantages. It has", he says, "conferred incalculable benefits on millions of people by reducing their burden of disease, improving their well being, and opening up vast territories to economic development. On the evidence available to WHO at present it would seem that, in spite of the adverse publicity DDT has received, there is at present no justification for abandoning this most valuable weapon in the fight against disease."

The mammoth 400 page report lists the work of WHO and highlights particular problems and successes of 1971. Cholera provides one of the less happy aspects of the year, three times more cases (148,000) being reported in 1971 than in 1970, with almost a sixfold increase reported from African countries. The number of countries reporting the disease (39) was, however, much the same as for 1970.

Reported smallpox cases rose from 30,000 to 50,000 in 1971, 25,000 of these coming from Ethiopia. This is accounted for by better recording in Ethiopia following the start of an eradication campaign there. The rise in the number of cases is the first since 1967, however, and the number of areas heavily affected by smallpox has dwindled considerably since then.

WHO also highlights the problems of cardiovascular disease, which, according to their own figures, was responsible

for 39 per cent of all deaths in men aged 25 to 64 years in a survey carried out in 29 technologically advanced countries in 1967. This loss, Dr Candau says, is far from inevitable. Too much food and too many cigarettes must be "vigorously combated" and early diagnosis and treatment of hypertension and heart disease must be extended.

The report also emphasizes that while much research is needed, many diseases, particularly of childhood—for example, measles, whooping cough, diphtheria and polio—could be better controlled simply by the application of existing knowledge.

In the course of 1971 WHO was responsible for running 1,900 projects throughout the world, 950 of those being collaborative projects with other organizations. The organization spent \$75.2 million during the year and published 101 papers, technical reports and official records.

ENVIRONMENT

Policy for Europe

THE cost of cleaning up pollution over a period of five to ten years and creating an environmental protection agency would be between 1 and 2 per cent of the gross national product of the European nations, according to Dr Hillard Roderick, director of the OECD Environment Directorate. The cost to industry would be about 10 per cent of its capital investment per year and some small companies would go out of business, but although these sums are huge they are not so large that they could not be sanctioned.

In such a situation, said Dr Roderick, the task of OECD is to harmonize environmental policies and to develop

guidelines for governments to follow; these guidelines, which are about to be agreed by governments, include the principle that the polluter and not the taxpayer should pay for the cleaning up operation.

Dr Roderick was speaking at a meeting of the European-Atlantic Group on "European Environment Policy", along with Mr Michel Carpentier, chief of the EEC Division of Environment, and Mr Sten Renborg, director of the Environment Directorate of the Council of Europe.

It emerged clearly from the meeting that there is no European environment policy as such, but there are plenty of organizations worrying about the environment, and, according to Mr Renborg, over the past few years the stage has been set for such a policy.

Mr Carpentier said that the strength of the EEC in environmental matters is that it can issue directives on the environment which advisory bodies cannot. And as the EEC's future is linked to "continuous and balanced expansion" the commission must be very concerned—"the solution to environmental problems is to some degree a condition of the EEC's survival".

But if a European environment policy does not yet exist, it is possible, with an enlarged community in the offing and the Stockholm conference in June, that one will emerge. Mr Renborg for one, although he said he is afraid that Stockholm will create more confusion than it will clear up, is looking forward to a ministerial conference on the environment in Vienna in March, 1973, when the Council of Europe ministers will meet to compare their problems and examine possible solutions. That, said Mr Renborg, "will be the next important milestone".

TEACHERS

More Come to Stay

ALTHOUGH the number of teachers in maintained and direct grant schools, further education establishments and colleges of education in England and Wales moved ahead by about 4.5 per cent to 404,000 in 1970, the proportion of graduates remained almost constant at 23.4 per cent. The number of mathematics graduates teaching in schools increased by 1.5 per cent to 5,820 between 1969 and 1970, while teachers with degrees in other science sub-

jects increased by 3 per cent to 15,601.

Since 1950 the percentage of graduate teachers has increased from 18.0 per cent to the present figure, and in that year 27.2 per cent of men school teachers had degrees, compared with 12.0 per cent of the women. In 1970 the comparable proportions were 32.3 per cent and 15.0 per cent respectively.

The report also reveals that a rise in the inflow of graduate teachers into schools has been accompanied during the past few years by a reduction in the outflow (Table 1), a situation which undoubtedly reflects the employment situation for graduates in general.

Table 1. Movement of Graduates in and out of School Teaching *

	1967-68	1968-69	1969-70
Gross inflow	13.6	14.2	15.3
Gross outflow	12.0	12.0	11.4
Net increase	1.6	2.2	3.9

*Flows and increases are percentages of the number of graduates at the beginning of the period. Source: *Statistics of Education*, 1970, 4 (HMSO £1.70).

NEW WORLD

Power and Pollution: Fuel for a Backlash

by our Washington Correspondent

LAST week was a bad time for environmentalists in the United States. Earth Week came and went without the fanfares of last year and was barely noticed among the reports from Vietnam and Apollo 16. Then the House of Representatives passed a bill which allows the Atomic Energy Commission to issue temporary operating permits for some power plants without subjecting them to full environmental review so as to avoid blackouts and voltage reductions in the summer peak period, and the Supreme Court decided that environmentalist groups cannot go to court to block projects that affect the environment unless the interests of their members are directly affected. Has the environmental backlash finally arrived in the United States?

Environmental groups still abound, the advertising industry is still cashing in on the bandwagon by presenting companies and products as ecology-conscious (eco-pornography is the word that has been coined for such practices) and politicians still pay plenty of lip service to the issue, but some environmentalists do fear that last week's unrelated events may be the harbingers of future trends.

Perhaps the event that has been most anxiously watched by environmentalists is the decision by the House of Representatives to amend the National Environmental Policy Act (NEPA) to give the AEC freedom to bring generators into service before a full environmental review. What concerns the environmentalist groups is that last week's decision may be the thin end of the wedge which will eventually emasculate NEPA.

For the past two years, the act has been the cornerstone of environmentalists' attacks on major government projects that are likely to lead to environmental degradation, and its provisions have been used to force major changes and delays in projects as diverse as the Trans-Alaska pipeline and nuclear generating plants. But, since the essential provision of the act is that agencies of the federal government must conduct a thorough review of the likely environmental consequences of each new project, environmentalists see the chief influence of NEPA as raising the level of environmental consciousness in the government. Already, in the first two years of operation of the act, more than 4,000 environmental impact statements have been prepared by government

agencies, and it is undoubtedly the most far-reaching piece of environmental legislation ever to reach the statute books.

As far as the Atomic Energy Commission is concerned, however, NEPA has become an administrative headache. The roots of the AEC's troubles come from an appeals court decision, relating to a power plant proposed for the Chesapeake Bay, which forced the agency to tighten up its environmental regulations considerably. The agency now suggests that administrative tangles have reached such an extent that thirteen power plants, which were under construction before NEPA came into force and which may be needed to meet peak load demands this summer, may be delayed from coming into operation because of the lengthy hearings procedure on NEPA impact statements.

Faced with power shortages this summer, when millions of Americans turn on their air conditioners, and in the following winter, and under the threat of even more dire power famines five and ten years hence, members of the House of Representatives were clearly caught in a cleft stick. If they refused to give the AEC temporary relief from the rigours of NEPA, and blackouts occurred this summer, the way would be wide open for an environmental backlash from an enraged public which would probably prefer the luxury of a working air conditioner to the more distant threat of environmental degradation from a power plant. Given the feeling against NEPA among some government agencies, however, especially the Federal Power Commission and the Department of Transportation, Congressmen were understandably reluctant to pass a measure that may set a precedent and eventually undermine NEPA itself.

In the event, the bill passed by the House of Representatives last week provides the minimum possible relief from NEPA for the AEC consistent with the threats of power shortages. Essentially it allows the AEC to issue interim operating licences to power plants under construction before January 1, 1970, so that they can be tested and ready to operate if the power situation demands it. Before they can be operated above 20 per cent of full power, however, the specific approval of the chairman of the AEC, the Environmental Protection Agency and the Council on Environmental Quality is required. In any case, the temporary licence would expire on

October 30, 1973, and all the plants must eventually go through full NEPA review.

The measure naturally has the support of the Administration, and it was proposed in the House of Representatives by some leading architects of NEPA itself. John Dingell of Michigan, for example, called the bill "a surgical cut" in NEPA "that would be self sealing", and many of his colleagues agreed with some reluctance that the bill is perhaps the least damaging to NEPA of a variety of other bills that have been suggested.

But some environmentalist groups have questioned the need for such a measure. A statement put out by the Sierra Club, for example, suggests that only three of the thirteen power plants covered by the bill are critical for meeting possible power shortages. These are the so-called Quad Cities 1 and 2 plants in Illinois and the Point Beach 2 plant on Lake Michigan. The Quad Cities plants have been held up by litigation, but the case has now been settled with the contractor agreeing to install extra cooling towers, and safety hearings have already been concluded for Point Beach 2. The Sierra Club thus argues that there is no immediate urgency to provide the AEC with temporary relief from the rigours of NEPA, and that the act is being needlessly weakened. The club also claims that James Schlesinger, chairman of the AEC, has agreed that settlement of the Quad Cities dispute has eliminated the need for relief from NEPA, but Schlesinger has since refuted the suggestion.

The environmentalists' ultimate recourse is to the courts, where they have won some stunning victories, not least of which are delay in the Trans-Alaska pipeline and a halt to the sale of oil leases off Louisiana. On the face of things, therefore, the Supreme Court decision last week, which suggests that environmental groups cannot go to court to protect the environment unless the welfare of their own members is at stake, should be a cause of great concern to the groups. The court decision, which was made in relation to a case brought by the Sierra Club which was attempting to block the development of an area of the Sierra Nevada by Walt Disney Enterprises, by a 4-3 vote stated that the organization had "failed to allege it or its members" would be harmed by the building of the \$35 million resort.

The Sierra Club has, however, taken the blow relatively lightly, since the decision merely implies that conservationist groups must recast their law suits

to back the cases of individuals who are directly affected by proposed projects. It will, however, now be necessary for individuals to put their own feelings to the test of the courts, which naturally requires a strong personal commitment, but the decision may soon be negated by a bill introduced into the Senate by Philip A. Hart of Michigan and George McGovern of South Dakota which would grant the required legal standing to such organizations as the Sierra Club.

Although the decisions by the Supreme Court and the House of Representatives may not be as damaging to the environmental movement as they seem at first sight, the greatest danger to the movement would be a loss of public support. The government's volte face and subsequent confusion over phosphates last year may have sown the seeds for an environmental backlash, but a greater danger would come from a head-on clash between personal luxuries and environmental values, which could arise from power shortages this summer. Last week's markedly reduced support for Earth Week may thus turn out to be the most important indicator of things to come.

SHUTTLE

End of Round One

by our Washington Correspondent

THE space shuttle won its first test of Congressional opinion last week with considerable ease: a move to defer funding for the enterprise picked up only eleven supporters and 103 opponents when it was put to the test in the House of Representatives. The occasion was a debate on the authorization bill for the National Aeronautics and Space Administration which would give the agency a budget of \$3,418 million for 1973—some \$50 million more than the Administration requested—and an increased stake in aeronautical research and development.

Although few people expected the House of Representatives to turn sour on the shuttle—there are too many dollars and lucrative contracts for the ailing aerospace industry at stake for congressmen to vote down the project in an election year—it was approved by a surprisingly large margin. A stiffer test of Congressional opinion will, however, come when the Senate debates the NASA authorization bill that is expected to be reported out of the Senate Committee on Aeronautics and Space Science by the end of April. The project has already picked up some influential opponents in the Senate, including William Proxmire, Edmund Muskie and majority leader Mike

Mansfield. But last year the same forces were defeated by a 22–64 vote in their attempt to delete funding for the shuttle, and this year they will not be able to point to the fact that the project will eventually cost \$12.8 million to back up their arguments about misallocation of resources: NASA has revamped its plans and reduced the cost of the shuttle to \$5,100 million (see *Nature* 235, 68; 1972).

The move to defer funding for the shuttle was led in the House of Representatives last week by Mr Les Aspin, a freshman Congressman from Wisconsin, who is following closely in the footsteps of his colleague in the Senate, William Proxmire. Aspin introduced an amendment to the authorization bill which would have delayed funding for the project for a year to give the National Academy of Sciences time to conduct "a full and complete study" of the shuttle, including its possible cost, scientific applications and economic impact. Aspin questioned the economics of the shuttle, pointing out that if it is to save any money on launch costs, there will have to be a considerable increase in the amount of payload orbited each year. With payload costs running at up to \$20,000 per pound, Aspin argued that the shuttle will eventually add up to a cost of between \$50,000 and \$60,000 million over the next ten or twelve years (an estimate that was backed up by calculations performed by Dr Ralph Lapp and inserted in the Congressional Record by William F. Ryan).

Aspin's arguments cut little ice with his colleagues, however, who argued that economic studies carried out for NASA indicate that the shuttle will save up to \$1,000 million a year in launch costs, and that a vote against the shuttle will be a vote against technological progress. The House of Representatives seems to have accepted the fact that NASA has staked its future on the shuttle, and is willing to let the agency go ahead with its plans.

Apart from the \$200 million earmarked for shuttle development, the authorization bill passed last week by the House reflected and even extended the Administration's avowed policies of stimulating research and development that is likely to provide practical or economic benefits. The funding recommendations increased the Administration's requests for expenditure on aeronautical research and development, while leaving the agency's other activities relatively untouched. The only project to be cut was the High Energy Astronomy Observatory, whose budget was reduced by \$4 million from \$68.6 million.

Although NASA can take some comfort from the fact that the shuttle has survived its first congressional test, its

budget is as yet far from decided. The Senate Authorizations committee has yet to report, and both the Senate and House Appropriations committees, which will recommend the actual amounts of money that Congress will make available, are still in the process of considering the agency's budget.

SOLAR PHYSICS

Future in Doubt

by our Washington Correspondent

COST overruns on NASA's next planned solar physics satellite may have put the satellite in jeopardy, and NASA officials will decide in the next few weeks whether or not to cancel the main contract for the project. If the satellite is scrapped, it will probably signal the end of the orbiting solar observatory (OSO) series of satellites, at least until the shuttle is available to place them in orbit, and it would force satellite-borne solar physics into a period of relative quiescence.

Called OSO-I, the satellite is scheduled for launch in November, 1973, although the launch date may already have slipped to early in 1974. A year ago, three more OSO satellites were planned, but the squeeze on NASA's budget forced the agency to defer decisions on two of them until next year, leaving OSO-I as the only solar physics satellite for which funding has been requested. Although Dr John Naugle, NASA Administrator for Space Sciences said, at a press briefing on the agency's 1973 budget in January "we have definitely not killed OSO-J and K," solar scientists are not optimistic about the chances of getting the satellites, and are therefore looking to OSO-I as the final and most important satellite in the series.

Although NASA officials admitted last week that OSO-I is up for review, they were reluctant to discuss the costs of the project or the likely outcome of their deliberations. Dr Goetz K. Oertel, chief of the NASA solar physics programmes, said that doubts about the future of OSO-J and K have confused cost estimates for OSO-I, but at present the satellite is still an approved programme and planning is going ahead on that basis. But a staff member of the House of Representatives Committee on Science and Astronautics, which has asked to be told in advance of any decision, said last week that he believes cost estimates for the satellite have doubled over the past few months. The committee is understood to have toyed with the idea of increasing authorizations for NASA's solar physics programmes, but decided instead to await results of the review.

Because of the importance of OSO-I

to solar physics, a decision to scrap the programme would be fiercely resisted by the solar physics community. Solar physicists have already been instrumental in resurrecting the programme once—when NASA's budget request was being prepared last year, the OSO programme was in danger of being terminated but a flood of letters and telegrams kept OSO-I alive and the other two still a possibility—and Dr Oertel said last week that interest in the satellite has not changed in the meantime. NASA officials will also have to bear in mind the fact that one of the chief experiments on the satellite is being financed by the French government, and that users groups are already being set up to plan experiments and share the data from the satellite. The groups include scientists from the USSR and other European countries.

Dr Elmo Bruner, of the University of Colorado, who is chief scientist for one of the main experiments aboard OSO-I, said last week that if the satellite is scrapped, it will be a "very, very serious loss to solar physics". Pointing out that OSO-I will probably terminate the OSO programme, Dr Bruner said that the satellite represents "the culmination of experiments on the sun up till now".

NATIONAL SCIENCE FOUNDATION

A Friend in the Senate

by our Washington Correspondent

SENATOR EDWARD M. KENNEDY, who has quietly adopted Emilio Q. Daddario's former role as friend for science on Capitol Hill, last week provided ample evidence of his friendship by recommending that the budget for the National Science Foundation in 1973 should be \$94 million greater than the Administration has requested. The suggestion is embodied in a bill which Kennedy introduced into the Senate last week that would give the Foundation a budget of \$740 million in 1973, most of the increase going to the NSF's applied research and education programmes.

The importance of the bill is that Kennedy is chairman of the Senate subcommittee which sets authorization levels for the National Science Foundation, and since the bill is cosponsored by four other members of his subcommittee, it is likely to form the basis of the subcommittee's recommendations for the foundation's budget for 1973. Kennedy has tentatively scheduled hearings on the bill for the first week in May.

Although the House and Senate authorizations committees are the least powerful of the four committees which deal with the NSF budget request—they merely set upper spending limits, while the appropriations com-

mittees recommend how much money Congress should actually make available—they are important for shaping the direction of the Foundation's activities and for conveying to the foundation the feelings of those members of Congress who bother to take an interest in everyday science affairs. As far as Kennedy is concerned, the message to the Foundation is loud and clear: it should not turn its back on supporting graduate students, it should increase its involvement in other educational activities, and it should also spend more on its programme of Research Applied to National Needs (RANN).

In detail, Kennedy's bill would give the Foundation an extra \$56.2 million for its education programmes, \$16.5 million for RANN, \$9 million for a programme designed to assist state and local governments to strengthen their scientific activities, and modest increases in funding for basic science. The increase in support for education programmes would be split in the ratio of \$18 million for institutional grants, \$17.8 million for graduate student support and \$20.4 million for other science education improvement activities. As for the RANN programme, Kennedy is recommending increases of \$12.6 million for energy research and \$3.9 million for earthquake engineering.

Kennedy's companion committee in the House of Representatives has also recommended increases in the NSF's support of science education and of graduate students (see *Nature*, 236, 258; 1972), although its suggestions are much more modest. The House of Representatives is expected to vote on the authorizations in the last week of April.

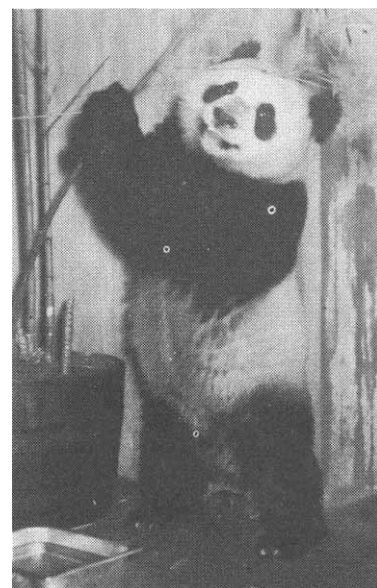
BIODIPLOMACY

Pandas from Peking

THE top secret security operation that brought two giant pandas to Washington at the crack of dawn on April 16 erupted in a blaze of publicity four days later when Mrs Patricia Nixon, the President's wife, formally accepted them as a gift from the Peking Revolutionary Committee. These two specimens of *Ailuropoda melanoleuca* have assumed more than zoological importance, for they have come to the National Zoo as symbols of the new-found trans-Pacific friendship. Presenting the beasts to Mrs Nixon, Mr Ting Hung, head of Peking's Bureau of Public Service, also offered greetings to the American people.

Rumours that the female, Ling-Ling, was nineteen years old, thus establishing a longevity record for giant pandas, were quashed by the announcement that both she and the male, Hsing-Hsing, are a mere eighteen months old. This news should bring joy to the hearts of conservationists who see in the Chinese

gift the opportunity to replenish the giant pandas of the western world. Until last week's arrival in Washington the only remaining examples of the species outside China were the ageing Chi Chi in London and An An in Moscow, both supplied by Peking in the late 1950s. (Chi Chi is in any case at death's door.) After unsuccessful attempts to mate them in 1966 and 1968, despairing zoologists were forced to resign the study of this species and its mating habits to their counterparts in Peking. They of course had access to the only remaining wild giant pandas, and seemed unlikely to relinquish any of them.



Ling-Ling the giant panda breaking off bamboo shoots in her new home at the National Zoological Park in Washington DC. (Photograph: Smithsonian Institution, negative No. 72-4365.)

The United States has not had a giant panda since 1951, and so the arrival of two healthy young specimens is welcomed by representatives of the World Wildlife Fund as well as by the politicians who see the operation merely in terms of top level diplomacy. Ling-Ling and Hsing-Hsing have arrived at such short notice that their new quarters are not ready, but a new cage is being built, and they will eventually have about an acre of ground which is intended to resemble their natural habitat. When these outdoor quarters are completed the two animals will meet for the first time, and within a year or two a great many people will be watching anxiously for signs of mating behaviour. Meanwhile it is to be hoped that Milton the musk ox, sent to Peking with his companion Matilda in a gesture of reciprocation, will recover from the nasal drips and hair loss that set in soon after his arrival.

NEWS AND VIEWS

Phytochrome, Hormones and Membranes

IN recent years, the view has been gaining ground that phytochrome, the proteinaceous photoreceptor responsible for a wide range of developmental responses of higher plants to light, operates in some way by regulating the permeability or other properties of cell membranes. In no small way responsible for this view has been the discovery by Dr Takuma Tanada of the United States Department of Agriculture Soils Laboratory at Beltsville, Maryland, that excised root tips of barley and mung bean can be caused to adhere to a negatively charged glass vessel upon irradiation with red light (*Proc. US Nat. Acad. Sci.*, **59**, 376; 1968; *Pl. Physiol.*, **43**, 2070; 1968). The tips adhere to the glass very rapidly and subsequent irradiation with far-red light causes their release. It has been shown that this adhesion response is probably attributable to very rapid changes mediated by phytochrome in the electrical potential across the root tips (M. J. Jaffe, *Science*, **162**, 1016; 1968) thus supporting the concept that phytochrome is either a component of the root cell membrane, or acts upon those membranes.

On page 460 of this issue of *Nature* Tanada extends these observations and demonstrates a most intriguing interaction between phytochrome, indoleacetic acid (IAA) and abscisic acid (ABA) in the control of root tip adhesion. It was already known that low concentrations of IAA were necessary for the adhesion response to red light but that above 10^{-8} M the hormone prevented adhesion. Tanada has now shown that ABA at very low concentrations has an exactly opposite action to that of IAA in preventing the release of the root tips by far-red light. Tantalizingly, Tanada also refers to as yet unpublished data showing that IAA and ABA act antagonistically in this response and that the inhibitory effects of IAA on adhesion can be reversed by ABA.

The effects of these two hormones on electrical potential are consistent with recent work on the rapidity of their action in other phenomena. For example, similarity between the effects of IAA and low pH on elongation growth of *Avena* coleoptiles (D. L. Rayle and R. Cleland, *Pl. Physiol.*, **46**, 250; 1970) could indicate that IAA might alter membrane characteristics, perhaps by the activation of a proton pump. Similarly, the rapid effects of ABA on stomatal movements (C. J. Mittleheuser and R. F. M. Van Steveninck, *Nature*, **221**, 281; 1969) and on the permeability of carrot discs to water (Z. Glinka and L. Rheinhold, *Pl. Physiol.*, **48**, 103; 1971) suggest a membrane site for the action of this growth regulator also. It therefore seems that research into three of the substances—phytochrome, IAA and ABA—which regulate growth and development in higher plants is converging on the plant membrane and one must expect a flurry of interest in membrane properties. One of the limiting factors, of course, is the technical difficulty associated with the preparation and purification of plant cell membranes. Anybody who develops a successful method for the large-scale preparation of plant membranes would deservedly earn the gratitude of a very large number of frustrated plant physiologists.

Another article in this week's *Nature New Biology* (**236**, 255; 1972) deals with yet another phenomenon controlled by phytochrome, but in this case with a much longer time course and therefore providing no additional information on the primary action of phytochrome. Nevertheless, this report by G. J. Acton of the University of Glasgow of a phytochrome-mediated increase in the levels of ribonuclease in etiolated lupin hypocotyls serves to underline the fact that the complexities of the phytochrome system are not yet fully explored.

Acton shows that the increase in ribonuclease is at first sight a typical phytochrome response in that it is potentiated by brief red light and this potentiation is negated by far-red light. What is particularly intriguing, however, is that reversal cannot be demonstrated with a narrow band-pass filter source with peak transmission at 729 nm (very close to the absorption maximum of isolated P_{fr} , the far-red absorbing form of phytochrome) yet is relatively easily achieved with a broad band source ranging from 735 to 1,300 nm. Although the data for the 729 nm source is only referred to in passing by Acton, its potential significance is such that the attention of other photomorphogeneticists should be drawn to it. Taken at face value it implies that P_{fr} in lupins has very different absorption properties from those known for isolated phytochrome, which if confirmed would necessitate a reappraisal of some hitherto accepted ideas.—H. S.

Catecholamine Synthesis

AS students of aminology well know, one of the few enzymes with high substrate specificity involved in the metabolism of the catecholamines is "tyrosine hydroxylase", which catalyses the first and rate-limiting step in catecholamine biosynthesis. Other enzymes such as L-aromatic amino-acid decarboxylase or monoamine oxidase, on the other hand, have a broad substrate specificity and are not involved exclusively in the metabolism of the catecholamines, dopamine, noradrenaline and adrenaline. The specificity even of tyrosine hydroxylase, however, has recently been called into question.

In a study of the partially purified enzyme from beef adrenal medulla, Shiman *et al.* (*J. Biol. Chem.*, **246**, 1330; 1971) demonstrated that the preference of this mixed function oxidase enzyme for L-tyrosine was highly dependent on the nature of the pteridine cofactor used. In the presence of certain synthetic pteridines, L-tyrosine was the preferred substrate, but with other pteridines such as tetrahydrobiopterin (the probable endogenous cofactor) L-phenylalanine was hydroxylated as least as rapidly as L-tyrosine by the partially purified adrenal enzyme. The report by Karobath and Baldessarini in a recent issue of *Nature New Biology* (**236**, 206; 1972) suggests that these two amino-acids may also be alternative substrates for catecholamine biosynthesis under normal conditions in mammalian brain.

Partially purified preparations of nerve endings ("synaptosomes") from the homogenized brains of five different mammalian species were found to be capable of forming labelled catecholamines when incubated *in vitro* with either tritiated L-tyrosine or L-phenylalanine. The rate of formation of labelled catecholamines from L-phenylalanine was surprisingly high—30–50 per cent of that observed with L-tyrosine. Because L-phenylalanine would have to undergo two hydroxylation steps to form labelled catecholamines, this would suggest that the rate of hydroxylation of phenylalanine may well be at least as rapid as that of tyrosine in nerve terminals in brain. When different concentrations of amino-acids were tested, both phenylalanine and tyrosine showed saturation kinetics with approximate K_m values of 5 μ M for each. There was, furthermore, evidence for mutual inhibition between the two amino-acids, with L-tyrosine being somewhat more effective in inhibiting the conversion of L-phenylalanine to catecholamine than *vice versa*.

These results suggest that under the conditions used, in which naturally occurring cofactors for hydroxylase would be conserved in high local concentrations within nerve terminals, which survive the homogenization procedure, either tyrosine or phenylalanine may serve as precursor for the biosynthesis of catecholamines in brain. Karobath and Baldessarini suggest that both phenylalanine and tyrosine may be hydroxylated by tyrosine hydroxylase in adrenergic nerve terminals, and that the endogenous cofactor in such terminals is probably tetrahydrobiopterin. If these results are substantiated, it may be that tyrosine hydroxylase may suffer the same fate as "DOPA-decarboxylase" and will have to be relegated more appropriately to "L-aromatic amino-acid hydroxylase".—From a Correspondent.

Hunting Poly A

LIKE the search for reverse transcriptase here, there and everywhere, the detection of polyadenylic acid sequences in messenger and other RNAs is currently a most fashionable pursuit. It is unlikely that Hadjirassiliou and Brawerman, when they reported in 1966 that a 10S RNA very rich in adenylic acid residues can be liberated from rat liver microsomes by deoxycholate, realized what activity they were going to stimulate. In the immediately following years only a small handful of workers, notably Lim and the Canellakis, pursued this finding, but soon after these authors reported in *Nature* (227, 710; 1970) that a poly A rich polymer is associated with rabbit reticulocyte, in other words globin messenger RNA, the ball really started rolling. Since 1970 it has been learnt that poly A is present in many eukaryotic cell messengers and polysomal RNAs, in heterogeneous nuclear RNA and in the messengers transcribed off vaccinia virus and adenovirus DNAs. Poly A has not, however, been detected in ribosomal and transfer RNAs.

These findings, not surprisingly, have led to the suggestion that the presence of a sequence rich in adenylic acid, presumably at either the 5' or 3' terminus of both termini, is a universal characteristic of messenger RNA translated in eukaryotic cells. And speculation about the possible function of the poly A tracts of messengers has in the absence of any data been rife; there have been

suggestions that the poly A sequences may play some part in the transport of RNA from nucleus to cytoplasm, and speculation that it may play some regulatory part in translation.

The genomic RNAs of the RNA animal viruses must, by definition, act either directly as messengers or as templates for the transcription of messengers. These RNAs therefore might be expected universally to contain poly A tracts, always assuming of course that the poly A sequences of messengers are stable and genetically coded and not added by some terminal addition polymerase after the messenger has been transcribed. Recent experiments, some of which have been reported in *Nature* and *Nature New Biology*, indicate, however, that the situation may not be that straight forward.

The work of Lai and Duesberg (*Nature*, 235, 383; 1972) and that of Gillespie, Marshall and Gallo reported in last Wednesday's *Nature New Biology* (236, 227; 1972) prove beyond dispute that probably all of the 30–40S subunit RNAs of the 60–70S single stranded RNA genomes of the RNA tumour viruses contain covalently linked poly A sequences with molecular weights of about 60,000. Among the non-oncogenic RNA animal viruses the situation is more confusing, however. It is clear from the work of these two groups that the double stranded, genomic RNA extracted from a myxovirus (influenza virus), a paramyxovirus (Newcastle disease virus) and a rhabdovirus (vesicular stomatitis virus) do not contain significant poly A tracts. Polio virus RNA (polio is a picorna virus) and Sendai virus RNA both contain significant amounts of poly A, however, and Johnston and Bose have recently reported (*Biochim. Biophys. Res. Commun.*, 46, 712; 1972) the detection of a 50–100 nucleotide sequence rich in adenylic acid in the RNA of Sindbis virus (an arbovirus).

It is at present difficult to rationalize all these findings into a simple comprehensive picture. But as Gillespie *et al.* point out it is beginning to look as though those viral RNAs which serve directly as messengers upon infection have poly A, whereas those viral RNAs which serve as templates for the transcription of messengers do not usually have poly A tracts or have only very short tracts.

If this correlation can be confirmed and extended it poses several fascinating questions. For example, do the messengers transcribed from the double stranded genomic RNAs of the myxoviruses, paramyxoviruses and rhabdoviruses which lack poly A, have tracts added by an end-addition polymerase? The possibility that poly A tracts are not genetically coded has to be seriously considered because not only do the nuclei of calf thymus cells contain an enzyme capable of this reaction but also adenovirus messengers contain poly A even though adenovirus DNA does not apparently contain poly T rich regions.

If the paramyxovirus, Sendai virus is anything to go by one might anticipate that poly A is added to myxo and paramyxo virus messengers. But if poly A is added to more messengers from the nucleus to the cytoplasm it may not be required by the messengers of viruses which replicate in the cytoplasm. Certainly Lai and Duesberg seem to favour the idea that the poly A is attached to tumour virus RNA to achieve its exit from the nucleus. But one can always point to the poly A in polio virus RNA which suggests that the tracts are required for some

step in translation instead of or in addition to any transport from the nucleus. And Gillespie *et al.*, not to be outdone, speculate that the particularly large amount of poly A in tumour virus RNAs may reflect some failure in cleavage maturation processes which result from infection by tumour viruses.

Obviously until more data about the function, origin and sequence of poly A tracts in messengers of cell and viral origin are forthcoming these speculations will abound. But with so many groups turning their attention to the hunt for poly A tracts it should not be long before at least some of these ideas can be eliminated. Determination of the sequence of a poly A tract would be particularly instructive and might throw much light on the central question of whether or not these tracts are genetically coded.—From our Cell Biology Correspondent.

MAGNETOSPHERE

Data from Imp 5

from our Magnetosphere Correspondent

THE parts of the Earth's magnetosphere above the polar caps are almost unexplored. It has long been expected that magnetosheath plasma would enter geomagnetic field lines passing near the "neutral point" which appears in theoretical models that ignore the existence of an interplanetary field. This may be even more significant when the interplanetary field is taken into account. Such plasma was reported last year and named the "cusp". The ESRO satellite HEOS 2 is the first spacecraft to have a highly eccentric orbit with its apogee at a high latitude and should give a systematic survey of the neutral point which it passes closely. Meanwhile, some magnetic measurements near the neutral point have been obtained from the satellite Imp 5 (Fairfield and Ness, *J. Geophys. Res.*, **77**, 611; 1972).

Imp 5 was launched in June 1969 and although its apogee is at quite a low latitude it has a near polar inclination. It is on the day side of the Earth in northern summer and its inbound passes have high northern latitudes at the magnetopause. Only data "confidently felt to be within the distorted geomagnetic field" are included in Fairfield and Ness's report, but these measurements fit theoretical predictions with a degree of variability which might be reasonably expected. Outbound passes are at low latitude and show the well known compression of the dipole field. Inbound passes at high latitude show weakening of the field and more variability in its direction, the average direction being roughly that for the dipole. Fairfield and Ness published detailed data for

parts of four orbits and they mark the boundaries found by Frank (*ibid.*, **76**, 5202; 1971) from his plasma and energetic particle detectors.

Orbit 5 is characteristic of middle latitudes and resembles observations at low latitudes. The plot suggests, however, that magnetosheath plasma and turbulence extend within the boundary exhibited by the magnetic data, and this situation possibly indicates day side reconnection.

The magnetometer was saturated in the cusp on this orbit, but the other orbits shown include the cusp and "polar cap" given by Frank. In the magnetic data the polar cap is characterized by quiet, the cusp by turbulence and the field strength is usually depressed more in the cusp than in the neighbouring polar cap. Large fluctuations were observed in the cusp on orbit 4 in spite of a low *Kp* value of 0+. On orbit 9 Frank finds a transition from magnetosheath to cusp, but the magnetic data show no marked change though they are noisy. The changes in magnetic field direction occur in the middle of the cusp.

Orbits 7 and 9 each show a large spike in declination located in the cusp. Fairfield and Ness interpret this in relation to a double layer of field aligned current and compare it with observations on a low altitude satellite (Armstrong and Zmuda, *ibid.*, **75**, 7122; 1970). They find good agreement between the magnitudes of the field perturbations, but the Imp 5 spikes should map into narrower layers than that studied by Armstrong and Zmuda. The signs of the perturbations agree, the current being downward at the higher latitude, so that the electric field in the ionosphere should correspond to an electric potential which is higher on the field line entering the ionosphere at the higher latitude.

FORENSIC SCIENCE

Microscopes and Crime

from a Correspondent

THE forensic scientist's most basic instrument is the microscope. Advances and techniques in forensic microscopy were the subject of a symposium on the microscope in crime detection at Micro 72, the international meeting organized by the Royal Microscopical Society at Oxford Polytechnic on April 10-14.

Working on the theory that "every contact leaves its trace", a common application in forensic science is the comparison of material found at the scene of a crime with material found on, or connected with, a suspect. The types of "material" include hair, fibres, blood, wood, paper, glass, paint and bullets. It was the problem of comparing the firing marks on bullets that led to the construction of the first split-field comparison microscope in the United States in the early nineteen-thirties and commercial production by Leitz from 1935. The two objects are presented to the two halves of the field of view and can be manipulated on separate stages, enabling an immediate comparison to be made of size, colour and structure. The various marks found on fired ammunition were described by Mr J. D. McCafferty (Metropolitan Police Laboratory). Assigning a bullet to a barrel depends on a comparison of the unique rifling marks, which are usually constant over many hundreds of rounds fired from the same barrel.

Mr B. R. J. Morgan (Home Office Forensic Science Laboratory, Cardiff) spoke of the problems of comparison microscopy on other materials. Here the value of the evidence often lies in a positive match between a large number of specimens, which individually may be quite common. With certain speci-

30 Doradus Observed in the Infrared

INFRARED observations which provide a clue to the structure of the 30 Dor nebula in the Large Magellanic Cloud (LMC) are reported by I. S. Glass in next Monday's *Nature Physical Science* (May 1).

The 30 Dor nebula is the most conspicuous object in the LMC at both optical and radio frequencies; Glass has found that infrared radiation from the same region comes from a source 1 arc min across centred on the nebula, which is 25 arc min in diameter. The equivalent radio source seems to be associated with a thermal source 4 arc min in diameter within a non-thermal source 24 arc min wide and an HII region of 45 arc min full width at half maximum. The whole complex is

similar at radio frequencies to the centre of our own Galaxy, and the new observations at 1.2 to 3.5 μm provide further support for this idea.

So observations at radio frequencies and at a few microns both now show that 30 Dor and the Sgr A complex at the centre of the Galaxy contain similar thermal and non-thermal sources. If similar results can be obtained at intermediate frequencies there will be considerable justification for regarding 30 Dor as the centre of the LMC, and hopefully further studies will reveal characteristics of 30 Dor and the LMC which will lead to a better understanding not just of that galaxy but also of galaxies in general and our own Galaxy in particular.

mens, a match means the identity of a number of physical properties which can be measured under the microscope. Mr R. Cook (Metropolitan Police Laboratory) showed how this applied to the range of natural fibres. Fluorescence, refractive index, birefringence and melting point can all be measured and compared, followed by infrared spectroscopy and even chromatography of the dye if the samples are still indistinguishable. In the case of natural fibres such as jute, hemp, sisal and coir, such tests cannot be applied and straight microscopy is all-important. Mr C. G. Jarman (Tropical Products Institute) stressed the importance of sound knowledge and experience for the worker in this field if reliable identification is to be achieved. Another example of tests performed under the microscope is the series of microcrystal tests which are highly specific and sensitive for drugs. Although long established, these tests are somewhat neglected in Britain and Professor E. G. C. Clarke (Royal Veterinary College) urged forensic scientists to consider the advantages of the microcrystal tests particularly in distinguishing closely similar compounds.

The interpretation of pictures from the scanning electron microscope, and their relation to optical micrographs of the same object, sometimes proves difficult for those applying the scanning electron microscope to forensic problems. Mr M. E. Taylor (Home Office Forensic Science Laboratory, Birmingham) warned against expecting too much too quickly from the instrument: as experience is gained, the value will increase. The Birmingham laboratory and the Metropolitan Police Laboratory are the only forensic science laboratories so far in Britain to have their own scanning electron microscopes, but the availability of cheaper instruments could increase sales. The leader in this field is undoubtedly the Cambridge Scientific Instruments' 'Stereoscan 600', sister to the big 'Stereoscan S4', but the two British rivals were also on view at the trade exhibition. Both Vacuum Generators and the confusingly-named Cambridge Scanning Co. offer scanning electron microscopes at prices which one could pay for an optical microscope.

All these small scanning electron microscopes can be used as electron microprobes by the addition of energy-dispersive X-ray analysis equipment. This seems particularly useful for the forensic scientist because the analysis for elements can be performed on particles or regions down to a few microns across, and is rapid, non-destructive and often quantitative. Here the problem of interpretation does not arise because the results can be related directly to conventional chemical analyses.

CYTOCHALASIN

Points of View

from our Molecular Biology Correspondent

It is seldom that two approaches to a problem come to such uncompromisingly opposed conclusions as those of Spudich and Lin (*Proc. US Nat. Acad. Sci.*, **69**, 442; 1972) and Forer, Emmersten and Behnke (*Science*, **175**, 774; 1972). The subject is important; it concerns the action of cytochalasin B, an alkaloid, which has the property of inhibiting a wide variety of contractile processes.

It seems to be generally agreed that the alkaloid operates by exerting some unwholesome influence on microfilaments. The drift of thinking in the field is that these microfilaments are often, and may turn out to be invariably, actin-like, for the composition and physical properties of the major protein constituent have been examined in a variety of cases and found to correspond closely to those of muscle actin. In many instances the capacity to bind heavy meromyosin (proteolytically truncated myosin) all along the filaments, in the characteristic arrowhead geometry found in muscle actin-heavy meromyosin complexes, has been demonstrated. It therefore seems by no

means unreasonable that cytochalasin B should bind to F-actin filaments, with some detriment to their structure or function. This Spudich and Lin seem to have shown.

The evidence looks clear enough: actomyosin, in conditions of ionic strength at which it is soluble, has a high viscosity, which falls sharply on addition of ATP. Spudich and Lin show an almost equally dramatic drop in viscosity when cytochalasin B is introduced. Likewise the addition of cytochalasin to myosin before the actin leads to an actomyosin of low viscosity. There is no detectable effect on myosin (or rather heavy meromyosin) alone, the intrinsic ATPase activity being unchanged. By contrast, the ATPase activity of actin-heavy meromyosin is inhibited to a maximum of 60 per cent. The cytochalasin thus apparently interacts with the actin, and indeed Spudich and Lin find that the viscosity of polymerized actin (F-actin) drops in the presence of the alkaloid. The monomeric G-actin is still able to polymerize in solutions containing cytochalasin, but only attains a viscosity equal to the relatively low final value observed in F-actin after addition of cytochalasin. Spudich and Lin also infer from the very rapid effect of cytochalasin on the actomyosin viscosity, compared with

Fingerprints of Maturing Ribosomal RNA

THE series of cleavages and methylation steps which result in the maturation of ribosomal 28S and 18S RNAs in mammalian cells have been exhaustively investigated, in particular by Penman and his collaborators who have followed the flow of radioactive label through the various RNAs associated with the nucleolus in HeLa cells and have drawn up a maturation pathway. Penman's group envisage that a 45S RNA, the product of transcription, is converted by a series of stepwise cleavages and methylation reactions to a 41S RNA, a 32S RNA which matures into the ribosomal 28S RNA and a 20S RNA which matures to the smaller ribosomal RNA.

Direct proof of this putative maturation pathway derived from convincing but nevertheless circumstantial evidence depends, of course, on determining the nucleotide sequences of the ribosomal RNAs and their putative precursors. That is a mammoth task, but as Maden, Salim and Summers report in *Nature New Biology* next Wednesday (May 3), enough partial sequence data can be obtained from fingerprints of nuclease digests of these various RNAs to confirm the pathway.

To cut a long story short, Maden and his associates have shown that the fingerprint of a digest of 45S RNA from

HeLa cell nucleoli is virtually identical to a fingerprint of a mixture of 28S and 18S ribosomal RNAs. There can be no doubt therefore that the 45S precursor not only includes the base sequences of both 28S and 18S ribosomal RNAs but also that all the methylated bases in the 45S precursor occur at sites which survive the maturation process and remain in the mature ribosomal RNAs. Similarly, 41S nucleolar RNA contains the sequences of both the mature ribosomal RNAs. By contrast 32S nucleolar RNA contains only the sequence of the 28S mature RNA and the fingerprint of nucleolar 18-20S RNA is almost identical to that of mature ribosomal 18S RNA.

One noticeable difference between the mature 18S RNA fingerprint and the 18-20S nucleolar RNA fingerprint is the presence of a methylated spot in the first but not the second. This indicates that the dimethylaminopurine in mature 18S RNA is formed after the RNA has left the nucleus. Zimmerman, who reported in 1968 a methylation step late in the maturation of 18S RNA, thought it occurred in the nucleolus, but these new data suggest it occurs in the cytoplasm. But, that aside, these fingerprint analyses fully confirm the proposed maturation pathway for HeLa ribosomal RNA.

the much slower change induced in F-actin, and from the apparent ability of myosin to displace cytochalasin from actin, that there is direct competition for actin between the two.

The extent of the diminution of the F-actin and the actomyosin viscosity under the influence of cytochalasin B is clearly not compatible with complete depolymerization. Either the filaments are shortened or their geometry is drastically changed, in such a way as to increase their cross-section. This is where Forer *et al.* enter the argument. Their work is based entirely on electron microscopy, and their pictures of actin filaments carrying arrowheads of heavy meromyosin, prepared in the presence and absence of cytochalasin B, are assuredly identical, at least to the innocent eye and the unbiased mind. They checked also that the release (or exchange) of the myosin from the filaments was not inhibited by the cytochalasin, by demonstrating the disappearance of the arrowheads after addition of ATP. Again actin filaments prepared from G-actin solutions containing cytochalasin presented an entirely normal appearance. Forer *et al.* conclude that cytochalasin B and actin (or actomyosin) do not interact in any interesting way.

Now it is, of course, true that the two studies do not measure directly comparable parameters; indeed the failure of chemical and structural measurements to make contact is no new experience, protein chemists and electron microscopists having often enough glared at each other eyeball to eyeball to no great effect. A reconciliation might in the present case result from electron microscopic measurements of filament lengths. If the effect of the cytochalasin is to chop down the filaments to a defined limit, this would raise some interesting new issues. A small distortion at the inter-strand contact sites could lead to a change in the helix repeat of the actin filaments with a consequent change in length, or if another protein were present an explanation in terms of a vernier effect on the limiting length could be invoked. Then again, one or other of the reported findings might just be wrong.

MAMMARY CARCINOMA

Domes of Replication

from our Cell Biology Correspondent
MOUSE mammary tumour viruses have of late caught the limelight not because of any great leap forward in understanding of their biology but because a particle which is structurally and biochemically similar has been detected in some human milks. It is hard to resist the suggestion that these human milk particles are the human counterparts of

the murine viruses and, by analogy with the murine situation, that they may have an aetiological role in human breast cancer. And because one of the chief justifications of research into lower mammalian RNA tumour viruses is the belief that there exist similar human viruses yet to be discovered, the detection of human milk virus has strengthened the conviction of the converted and won a few new converts into the bargain.

As luck would have it, however, mouse mammary tumour viruses are far more difficult to experiment with than the murine sarcoma and leukaemia viruses, not least because nobody has yet succeeded in infecting and transforming cultivated cells with mammary tumour virus. Inevitably, therefore, assays of this virus involve slow, costly experiments with mice and the extensive use of electron microscopy, although new reverse transcriptase assays are promising. The search for cultivated cells which support the replication of exogenous mammary tumour virus to

yield infectious progeny and which are transformed by the virus continues, but the recent work of McGrath, Nandi and Young (*J. Virology*, **9**, 367; 1972) raises the possibility that the search may inevitably be fruitless.

McGrath, Nandi and Young have placed in culture acinar tissue of primary mouse mammary tumours, which were known to be producing mammary tumour virus, mammary epithelial cells from pregnant BALB/cfC3H mice, which although not bearing tumours were infected by mouse mammary tumour virus, and, finally, mammary epithelium from uninfected healthy mice. When insulin and cortisol are added to these primary cultures the cells organize into three-dimensional structures, called domes, which appear to be *in vitro* analogues of mammary gland acini. The domes, for example, produce abundant casein, and their cells, if from animals infected with mammary tumour virus, support the replication of the virus in culture. By contrast, under a similar culture regi-

Enhanced Agglutination of RNA Transformants

It is now generally agreed that cells transformed by DNA tumour viruses and chemical carcinogens are more readily agglutinated by the lectins wheat germ agglutinin (WGA) and concanavalin A (Con A) than are the parental untransformed cells. The molecular basis of this is, however, obscure and is currently vigorously disputed.

Apparently, transformation by these agents results in a change either in distribution or number, or both, of the surface glycoproteins to which lectins bind. Of course, if this change in the cell surface, which is detected by agglutinins, has any general physiological significance, one might expect it to be common to all transformed cells irrespective of the nature of the transforming agent, and in *Nature New Biology* next Wednesday (May 3) Burger and Martin report experiments which, contrary to those of Moore and Temin (*Nature*, **231**, 117; 1971), indicate that that is indeed the case.

Last year Moore and Temin reported that, unlike rodent fibroblasts transformed by polyoma virus or SV40, chick and rodent fibroblasts transformed by several RNA tumour viruses are no more susceptible to agglutination by WGA and Con A than are the corresponding untransformed cells. This disturbing result seemed, on the face of things, to cast grave doubts on the idea that the surface change detected by lectins is causally related to the changed pattern of growth of transformed cells. It occurred to Burger and Martin, however, that the increased amounts of

mucopolysaccharides, in particular hyaluronic acid, which accumulate on the surfaces of cells transformed by Rous sarcoma virus (RSV) might interfere with the binding of lectins and agglutination.

Burger and Martin therefore exposed cells to hyaluronidase to strip any masking hyaluronic acid and then measured the agglutinability of untransformed chick fibroblasts and cells transformed by the temperature sensitive mutant of RSV isolated by Martin. This mutant has a temperature sensitive lesion in a gene, the expression of which is required to maintain a cell in the transformed state. They found that after treatment with hyaluronidase untransformed cells remain relatively unsuspceptible to agglutination. Cells transformed by the mutant and grown at its permissive temperature are, however, rendered much more susceptible to agglutination by hyaluronidase digestion, but identical cells at the non-permissive temperature are not readily agglutinated even after exposure to the enzyme.

In other words, transformation of fibroblasts by RSV, and by implication other RNA tumour viruses, leads to at least some of the changes to the cell surface caused by transformation by DNA tumour viruses. Furthermore, with both RNA and DNA tumour viruses the maintenance of this changed cell surface depends, however indirectly, on the continuous expression of at least one viral gene. Essentially similar conclusions have, of course, been reached by Capeller and Doljanski (*Nature New Biology*, **235**, 184; 1972).

men cells explanted from serially transplanted mouse mammary tumours, which during the serial transplantations have lost the capacity to develop *in situ* an acinar organization and do not *in situ* liberate virus, fail to form domes and also do not support detectable replication of mouse mammary tumour virus. In short, the capacity of murine mammary epithelial cells to support replication of mammary tumour virus is positively correlated with the cells' capacity to assume an acinar organization.

Cells which can form acini and domes, irrespective of whether or not they are infected and "transformed" by mammary tumour virus and are supporting its replication, respond in culture to contact inhibition of multiplication. By contrast the serially transplanted mammary tumour cells, which fail to form acini or domes and do not produce virus, behave as do transformed fibroblasts, overgrowing each other, continuing to mitosis and reaching high saturation densities. In short, transformation of mammary epithelial cells does not apparently immediately result in loss of contact inhibition. As McGrath *et al.* comment, this fact may in part explain why attempts to detect transformation *in vitro* with the mouse mammary tumour viruses have universally failed.

The growth of primary mouse mammary tumours, they argue, probably results from a failure of growth control at the tissue rather than the cellular level of organization, the difference between normal and tumour tissues lying in the number of acini that develop rather than the number of mitoses of any particular cell. McGrath *et al.* suggest that in primary tumours the progenitor cells of acini are apparently unable to recognize "spacing regulations imposed on acinar growth which normally fix an upper limit to the number of acini which can occupy a given area". If that is the case it will indeed be difficult to devise an *in vitro* biological assay for mammary tumour viruses, but it would be interesting to test whether or not the cells in domes or cultures of mammary epithelial cells from uninfected healthy mice can be successfully infected by exogenous mammary tumour virus, and then support virus replication, as do the domes in primary cultures of cells from infected animals.

MACHINE PERCEPTION

Patterns and Pictures

from a Correspondent

A CONFERENCE on machine perception of patterns and pictures was organized by the Institute of Physics' Physics in Industry Subcommittee in collaboration with the Institution of Electrical Engineers and the National Physical Labora-

tory at Teddington from April 12 to 14.

Most of the contributions assumed as their framework the standard four-stage paradigm of pattern recognition. First, the visual or auditory signal to be recognized is measured. In the pre-processing stage, various operators are applied uniformly to the signal to reduce its complexity. In stage three, features are extracted from the signal that will, it is hoped, be crucial in classifying the input. The final stage takes a representation of the input as a point in multidimensional feature space and classifies it by various linear, piecewise linear or non-linear decision techniques. The standard video measuring devices are flying spot scanners, television cameras or solid state arrays with the hope of electronically-deflected laser scanning in the near future. The recognition logic is usually designed on a computer but implemented in hard-wired form.

Mr M. Nadler (Honeywell-Bull, France) reviewed the optical character recognition technology which is available and concluded that although machines for reading specially designed fonts at high speed are now being installed, replacing many key punchers,

their high cost is preventing wider use. Two contributions described the Post Office's use of orthodox pattern recognition techniques to read machine printed post codes but nobody has yet designed a satisfactory system for handwritten material. Dr J. R. Ullman (National Physical Laboratory) suggested that the fault may lie in a paradigm that does not adequately express relationships of dependency between the features.

In a review of biomedical applications, Dr D. Rutovitz (MRC Cytogenetics Unit, London) attacked the academic tendency to conduct idealized computer experiments and emphasized the importance of good preparation and automatic presentation of specimens for computer analysis. Automatic chromosome analysis is now competitive with human analysis but many other possible applications, such as the analysis of fibrous sections, are far from being realized.

Mr F. Billingsley (Jet Propulsion Laboratory, Pasadena), who is responsible for some of the consciousness-expanding photographs from the National Aeronautics and Space Administration, discussed picture processing in the space programme. This work in

Effect of Anti-kappa Antisera on B Cells

THE search for chemical or biological agents which act specifically against B or T lymphocytes proceeds apace. The early studies of Reif and later of Raff and their colleagues have successfully established the θ -antigen as a characteristic of T cells and anti- θ antisera as the means by which destructive elimination of T cells from a given cell population can be achieved. The presence of complement receptors has been proposed as an attribute of B cells by Nussenzweig and his colleagues and indeed the possession of surface bound immunoglobulins is widely accepted as a B cell rather than a T cell characteristic.

Miller and his colleagues accepted this character as a starting point, and in next Wednesday's *Nature New Biology* (May 3) they describe experiments in which the light chain component of the surface immunoglobulins of B cells is attacked by heterologous anti-kappa antisera.

Ninety-five per cent of the light chains of mouse immunoglobulins are of kappa type, and Miller *et al.* prepared a series of rabbit anti-mouse kappa antisera using Bence Jones proteins from various mouse myelomas as their immunogens. The antisera, after absorption with mouse thymocytes, were tested against various kinds of mouse thoracic duct cells; first, normal mouse cells; second, cells from nude mice (as a source of B cells); and, third, cells

from irradiated F_1 mice that had, four days previously, received an intravenous injection of parental cells. In earlier experiments it was shown that these last cells are a pure T cell population. The anti-kappa antiserum killed a small proportion of cells from normal mouse thoracic duct lymph, nearly all the cells from nude mice and very few of the cells from the hybrid animals. The studies were carried out in conjunction with other tests for T cells (anti- θ) and B cells (the capacity to bind immune complexes). The results of all three tests were very similar, and Miller and his associates conclude that anti-kappa antisera are useful diagnostic and selective agents for B cells.

Not all Miller *et al.*'s anti-kappa sera, however, were of the same potency (the same is true of anti- θ). Nevertheless, there was a rough correlation between anti-kappa binding capacities of the various cells tested after incubation with anti-kappa sera and, although in the main B cell activity was depleted in various transfer systems, it proved impossible to deplete spleen cell populations of the capacity to respond to sheep erythrocytes. Miller *et al.* reject the possibility that B cells in the spleen do not have attackable surface immunoglobulins and conclude that B cell activity can be generated from stem cells in the transferred spleens which are not susceptible to the cytotoxic effects of anti-kappa antisera.

image restoration and enhancement enabled the removal of many of the effects of the Martian dust storms from the early Mariner 9 pictures. The Land Area Remote Sensing project at Purdue University uses frequency transformations of aerial and satellite photographs of the Earth to classify crops being grown.

A typical high energy physics laboratory produces half a million photographs a year, so physicists have been forced to automate their data analysis procedures. Mr D. G. Royston (Argonne National Laboratory) discussed the several systems designed in the past decade and concluded that the most successful are interactive rather than fully automatic.

Dr M. B. Clowes (University of Sussex) described recent computer programs for robot vision. These programs interpret the picture using knowledge of perspective projection and various hypotheses about the nature of objects, usually opaque polyhedra, and the relationships such as concatenation and support that those objects can enjoy.

At this conference there seemed to be a feeling that the pattern recognition paradigm is almost exhausted. That paradigm derives from taxonomy, with its emphasis on sorting patterns into classes. The analogy may be pursued. Pattern recognition is to machine perception as taxonomy is to modern biology. Further progress in machine perception will only be achieved by concentrating on the basic truth that pictures and sentences communicate meaning. A partition of multidimensional feature space is not an adequate representation of semantics, as workers in artificial intelligence have long realized.

CANCER RESEARCH

Treatment of Tumours

from a Correspondent

PROFESSOR J. F. FOWLER (CRC Research Unit in Radiobiology, Mount Vernon Hospital), as chairman, opened the twelfth and last meeting of the series on principles of experimental and clinical oncology at the British Museum (Natural History) on March 27 by outlining the bases of radiotherapeutic treatment of cancer.

Professor Fowler said that there are in Great Britain each year about 250,000 new cancer patients of whom 100,000 can either be cured or their disease can be brought under control until they die of other causes. About half of all patients are subject to surgery, half to radiotherapy, some to both. Patients of all kinds could be given additional chemotherapy. The adoption of high voltage generators of ionizing radiations has made a very substantial improve-

ment in the treatment of some forms of cancer; since 1940, for example, the five-year survival of patients with Hodgkin's disease has changed from 5 per cent (untreated) to 73 per cent in 1970 almost entirely as a result of the successful use of radiotherapy. The advantages of radiotherapy are that a fair degree of site localization is possible but there are always difficulties in controlling the damage to normal tissues.

It was clear, Professor Fowler said, that combinations of treatment are likely to be the most beneficial, but a distinct handicap in the way of achieving such an ideal is the lack of basic biological understanding of the relationship between malignant and normal cells. (This reiterated a theme which has cropped up several times during this series of meetings.) Professor Fowler stated that oxygen is the biggest single factor known to potentiate the damaging effect of ionizing radiation and that there is much to learn about the judicious use of hyperbaric oxygen as an adjunct to radiotherapy.

Dr M. Berenbaum (St Mary's Hospital, London) said that the basis

of chemotherapy is that cells differ from each other. He approached the problems of the chemotherapist by outlining the various ways in which a drug can affect a malignant cell. First, the drug has to get in and surfactants are likely to be useful in assisting this. Once in the cell, some form of activation is likely but it should also be realized that detoxification is another possibility. It is sometimes possible to utilize the dependence of cells on a particular metabolite to attack them, but again both repair mechanisms and alternative metabolic pathways may exist. Various studies have been made in experimental animals of drug sensitive and drug resistant tumours and also of the differing sensitivities of various normal tissues to drugs. The surviving fraction of a tumour may depend on the proportion of cells in cycle and Dr Berenbaum felt that with increased knowledge of such phenomena it might be possible to predict for any particular tumour what is likely to be the best chemotherapeutic treatment.

Mr I. Burn (Hammersmith Hospital, London) pointed out that treatment of

More on Tasman Geosyncline Tectonics

WHEN Oversby reported the first analysis of the Tasman Geosyncline (or Tasman Orogenic Zone) in terms of the new global tectonics (*Nature Physical Science*, **234**, 45; 1971), he admitted that his interpretation may not gain universal acceptance but hoped, nevertheless, that it would provoke discussion. In next Monday's *Nature Physical Science* (May 1) Solomon and Griffiths take up this challenge by extending the analysis to cover the whole orogenic belt (Oversby limited himself to consideration of the southern part).

Oversby envisaged that the southern part of the Tasman Geosyncline had been subjected to several tectonic cycles during the late Precambrian and Palaeozoic. Each cycle was accompanied by a time lag between the onset of plate motion and its manifestation on the continental side of the trench complex which includes the present geosyncline, and another lag between the end of motion and its final effects. Even so, each successive cycle seems to have overlapped the previous one both in time and space. The relatively mild tectonic activity and the lack of marked topographic relief in the region then led Oversby to suppose that the downwelling oceanic plate was moving westwards only very slowly; and the eastward younging of plutons was taken to indicate an eastward migration of the downwelling plate margin. Such a migration would presumably explain the predominance of steep structures in the area.

Solomon and Griffiths relate the Tasman Orogenic Zone to both New Zealand and Antarctica; they claim that the Cambrian to early Ordovician history of the zone is best revealed in Antarctica and Tasmania, and the Ordovician to Permian history in New South Wales and southern Queensland (namely, the Australian mainland). In general terms, what emerges from their analysis is that throughout the region there was a progressive eastward development; but continental margin growth by sedimentation was complicated by the probable existence of crustal blocks to the east. Subduction (trench) zones decrease in age in the easterly direction; but the Carboniferous and Permian sites in the north are separated by the continental crust of the Lord Howe Rise. This block, of course, could have developed by sedimentation from the continental margin; but in the relatively short period represented by the Carboniferous-Permian this seems unlikely.

Solomon and Griffiths suggest that the block is a fragment broken off from the Australian Shield. If so, it is clear that the subsequent collision of the Lord Howe block with the Australian margin would have severe implications for the continued growth of that margin—and these are spelled out by Solomon and Griffiths with supporting evidence from geological indicators—until, at a later stage, subduction and continental margin sedimentation became established to the east of the block.

solid tumours is the principal burden on the surgeon. The techniques for many forms of surgery practised on tumours today were developed in the second half of the nineteenth century after Lister's discovery of the use of antiseptics. There was no doubt that since that time tremendous improvements have been made in the survivals of some cancer patients subject to surgery but, nevertheless, Mr Burn saw that there is much room for improvement. Early diagnosis was without doubt the most useful aid to the surgeon; at present far too many cases of advanced cancer are operated upon.

AGEING

New Interest

from our Cell Physiology Correspondent

ALTHOUGH familiarity undoubtedly breeds contempt, this cannot be the only reason for the indifferent attitude of many people towards research on ageing. Possibly the way in which the problem is casually dismissed arises from an appreciation of some of the technical difficulties involved and a reluctance to undertake experiments which may take years to complete.

Almost seven years ago a short cut to this problem was suggested, namely that of ageing "*in vitro*", but although the evidence for a relationship between the finite life span of human diploid fibroblasts in culture and ageing "*in vivo*" is now very strong, there still does not seem to be a closer understanding of the basic principles involved.

Two recent articles in *Experimental Cell Research* arouse optimism, however, and suggest that interest in this field may be picking up. Lima and Macieira-Coelho (70, 279; 1972) have examined the changes associated with discontinuity in growth of chick embryo fibroblasts and they conclude that along with human and bovine diploid strains, these chick embryo cells may be used to study ageing *in vitro*: the cells can be passaged about thirty times every second day, so that the total life span is complete in less than three months, which is about half the time that human diploid fibroblasts take to die.

Apparently there are some differences between the ageing patterns of these chick fibroblasts and the human ones, for the time taken for chick cells to enter DNA synthesis does not change throughout the life of the cells and does not slow down even when the saturation density is declining. During the later passages, DNA synthesis is not followed by a doubling of the cell population, and Lima and Macieira-Coelho give two possible reasons. Like the ageing fibroblasts of other species, it is possible that the G₂ period is prolonged but it is

also likely that the ageing cells may have an increased sensitivity to cell crowding and may detach from the glass. This last suggestion indicates that ageing in chick fibroblasts has not quite the same characteristics as those of human and bovine fibroblasts and is supported by the fact that increasing numbers of cells are found in the supernatants of ageing chick cultures.

Many similarities exist, however, for apart from the decline in growth rate, chick fibroblasts also show the characteristic increase in cell size during the later passages. No doubt the easiest explanation of this phenomenon is to assume that it is merely a reflexion of the reduced potential for cell division, but the increase in size in this instance is not associated with a decrease in the capacity for division; the significant decline in saturation density occurs at a much later passage. This dissociation of ageing parameters may seem to be difficult to explain but it does suggest that ageing *in vitro* is not the result of any single event but is rather the end process in a sequence of changes.

In the same issue (*ibid.*, 436) Goldstein and Lin show that it is possible to rescue senescing cells, in this case human ones, by hybridization with hamster kidney cells. Although their results do not provide any positive evidence for the nature of ageing, one or two ideas

on the subject lose their plausibility. The system obviously provides a convenient method for obtaining inter-specific hybrids, for it is possible to select against both parental cells using 6-thioguanine resistant cells of the BHK line and senescent human fibroblasts, incapable of further cell division. Hybrid clones have been obtained which have been subcultured through more than a hundred cell divisions so that theories which are based on ideas of irreversible cross-linking of DNA, irreversible histone binding or dominant genetic ageing factors would appear to be an unlikely cause of ageing *in vitro*.

Goldstein contends that ageing is a quasi-differentiated event which is amenable to reprogramming, but he does not suggest any mechanism which might bring this about. Indeed, it would seem possible to interpret the results in favour of either of the two basic theories of ageing. Genetic reprogramming may well occur but it is also possible that accumulated faults in informational macromolecules may be diluted and "repaired" by the more accurate macromolecules originating from hamster kidney cells. Although it is difficult to think of a definitive experiment which might distinguish between these two possibilities, curiosity leads one to wonder what might result if two senescent cell cultures were hybridized.

Basis of Steroid Resistance

ONE of the great obstacles to the treatment by chemotherapy of people suffering from chronic diseases such as autoimmune disease and cancer is the tendency of the patient to develop resistance to the therapeutic agents. Presumably the population of target cells in the body is continually throwing up variants which are more resistant to the drugs being used than the rest of the population, and in the presence of the drugs such variants find themselves at a selective advantage and consequently come to predominate in the population.

The way in which somatic cell geneticists select drug resistant variant cells by passaging their cells in culture media containing a particular drug, the concentration of which is gradually increased during the selection procedure, may be a comparatively simple model of the situation faced by the chemotherapist. That at least is the contention of Rosenau, Baxter, Rousseau and Tomkins, whose analysis of the basis of resistance of cultivated mouse lymphoid cells to glucocorticoids is reported in *Nature New Biology* next Wednesday.

Glucocorticoids cause lymphoid cells to lyse and for that reason they are used to treat cancer and other diseases of the lymphoid system. Cultivated mouse

lymphoid cells are also susceptible to lysis by these steroids and steroid resistant variants can easily be isolated. Tomkins and his colleagues have recently developed cell free systems which allow them to measure two steps in the interaction of lymphoid cells with glucocorticoids; namely, the initial step of the binding of the steroid to a cytoplasmic receptor and the second step, the interaction of the receptor-steroid complex with the nucleus of the target cell. Using dexamethasone as the challenge steroid they have compared the extent of its binding at these two steps in cell free systems obtained from dexamethasone sensitive and resistant cells.

Rosenau *et al.* find that the binding of the steroid by cytoplasmic extracts of resistant cells is significantly less than the binding observed with similar extracts from sensitive cells, and this is not the result of an inhibitor of binding in the resistant cell extracts. Nuclei of resistant cells bind the receptor-steroid complex almost as well as the nuclei of sensitive cells. It seems, therefore, that the resistance of cultivated mouse lymphoid cells to lysis by glucocorticoids results from a change at the cytoplasmic receptor level rather than at the nuclear receptor level.

Pollution and Worldwide Catastrophe

JOHN MADDOX

John Maddox discusses the threats associated with environmental change, with special reference to the organo-chlorine pesticides.

POLLUTION is understandably an important ingredient of the present anxiety about the condition of the environment. In the late 1950s, it became apparent that fall-out from nuclear weapons tests might be a serious cause of genetic mutation among people and that the damage done would not be confined to the immediate locality of the explosion but, because of the dispersion of radioactive materials in the upper atmosphere, would be for all practical purposes worldwide. Although the political manoeuvres which led to the partial test-ban treaty in 1964 were complicated, there is no doubt that governments were already sufficiently concerned about the environmental hazards of fall-out to seek some common policy of self-restraint. At the time, fall-out was a powerful stimulant of public anxiety not merely because of the unfamiliarity of its biological effects—simple toxicity is familiar enough, but genetic mutations are another thing—but also because of the worldwide nature of the phenomenon. There is, after all, nothing in international law to regulate the ways in which one nation by its activities can knowingly damage populations elsewhere.

In 1962, before the signing of the partial test-ban, Rachel Carson's *Silent Spring*¹ drew attention to another novel cause of potential biological damage. Her protest against the extravagant use of pesticides in American agriculture was sensible and timely, for by then there was a long list of illustrations of how the immoderate use of pesticides had sometimes done unexpected damage and had sometimes even been counterproductive. But *Silent Spring* went further by suggesting that persistent pesticides such as DDT accumulated in body fat were a likely cause of cancer of the liver and even that the increasing incidence of leukaemia among children in the United States during the 1950s was linked with the increasing use of persistent pesticides and not, as she might have proposed, with the increasing use of antibiotics. With the first reports² during the 1960s of the occurrence of pesticides in the carcasses of Antarctic birds, it is understandable that DDT and similar chemicals should have succeeded radioactive fall-out as harbingers of worldwide catastrophe.

Since the appearance of *Silent Spring* in 1962, several other polluting agents have attracted attention in the scientific community and elsewhere. The toxicity of heavy metal ions, long suspected, has been more widely feared with the development of analytical techniques for the measurement of small quantities of these materials. In the past few years, contamination of the environment by mercury has been a particular cause of concern. With the discovery of small amounts of mercury in ice cores recovered from the Greenland ice cap, it has been possible to fear that mercury may share with DDT the property of wide dispersal. Then there have been calculations which suggest that the accumulation of carbon dioxide in the atmosphere as a result of burning fossil fuels in industrial plants and generating stations might affect the temperature of the surface of the Earth or otherwise change the climate. More recently,

it has been suggested that the discharge of heat from generating plants may be greater than can be accommodated by the atmosphere of the Earth some decades from now, especially if the rate of energy production increases at the pace which has been prevalent in recent years. In short, there is a great number of potential mechanisms by means of which worldwide catastrophe might be caused. At present, however, the uncertainties which attend an accurate calculation of these effects are so many and so great that, as will be seen, fears of imminent catastrophe are unfounded and, in some cases, even fears of more distant worldwide damage, predicated as they are on assumptions that there will be no significant changes in present practices, are premature, to say the least.

Worldwide environmental threats are to be distinguished from those which are more localized, either within nations or within groups of nations. In advanced societies, pollution of air and water is, of course, a familiar accompaniment of industrial activity, while the misuse of land can be traced back to the pre-industrial era and even to primitive communities. Countries such as the United Kingdom are proud of the way in which, in the past two decades, it has been possible to reduce substantially the amounts of smoke and sulphur dioxide in the atmosphere—in Britain, for example, the concentration of smoke in the atmosphere has decreased from 170 $\mu\text{g m}^{-3}$ in 1957 to 60 $\mu\text{g m}^{-3}$ by the late 1960s, while the total amount of smoke emitted from British chimneys has decreased from 2.31 million tonnes in 1953 to 0.93 million tonnes in 1968. The total quantity of sulphur dioxide emitted in Britain has been virtually static since 1960 but, because of better dispersion, the average concentration near the ground has declined from 175 $\mu\text{g m}^{-3}$ in 1958 to 110 $\mu\text{g m}^{-3}$ in 1968 (ref. 3). Moreover, according to the Royal Commission on Environmental Pollution, there is no reason why these trends should not continue in the 1970s, and exactly similar results are reported from the United States⁴, where the cost of air and water control measures now in hand is estimated to be the equivalent of 2 per cent of the Gross National Product by the late 1970s (ref. 5).

For the most part, localized sources of pollution raise problems which are national and largely economic in character. It is a question of regional politics to decide to what extent industrial costs should be increased so as to provide a more desirable environment. In the United States, there is an increasingly sophisticated tradition of calculating the cost of creating some desired degree of freedom from pollution and, even though there may be occasions on which decisions made may be economically irrational, these are for practical purposes problems which political societies can hope successfully to tackle. It is unfortunate that most discussions of localized pollution have been innocent of economic considerations, with the result that many advanced countries find themselves tackling obvious problems at considerable cost when more important sources of pollution might be contained more cheaply, and that little allowance is made for the great differences between the economies of developed and developing countries. To be sure, there are also circumstances in which several nations share a common pollution problem; there is, for example, common concern in Western Europe about pollution of the Rhine, and Scandinavian countries are increasingly inclined to think that sulphur dioxide from the west may be damaging to plant life. But

these are in principle, if not yet in practice, problems which could be regulated by sufficiently well ordered international institutions.

In the nature of things, the prospect of worldwide environmental transformations is a less tractable problem. Everybody knows how difficult it has been to regulate the hunting of whales in the South Atlantic through the International Whaling Commission, and the hoped-for agreement at the United Nations Conference on the Human Environment that there will be a ten-year moratorium on whale hunting is only a substitute for balanced exploitation of common resources. What follows is an account of one of the sources of environmental change which have recently been considered threats to survival—persistent organochlorine pesticides.

Between 1944 and 1968, a total of 1.2 million metric tons of DDT were manufactured in the United States, and although accurate figures are not available for other manufacturing countries, it has been estimated that the total production of DDT is of the order of 2.0 million metric tons. Since the early 1960s, the production of DDT in the United States has been declining and in 1968 was at 63,000 tons, roughly 75 per cent of peak production. There is no means of knowing whether the decrease of production in the United States has been made good by increases elsewhere, but the present rate of production of DDT is supposed⁶ to be of the order of 100,000 tons per year. The more toxic chlorinated hydrocarbons in the aldrin-toxaphene group, introduced in the mid-1950s, were in 1968 being manufactured at the rate of 52,000 tons a year in the United States and at a smaller rate elsewhere. All the authorities which have examined the problems of persistent pesticides in the environment agree that the lack of more accurate information about the production of these chemicals, and the chemically similar polychlorinated biphenyls, is an impediment to the proper understanding of how they are diffused in the environment (see, for example, ref. 7).

The way in which DDT accumulates in the body fat of human beings and marine animals has been one of the most powerful stimulants of public anxiety, even though the buffering effect of this phenomenon is no doubt at least partially responsible for the relative immunity of such animals to the toxic effects of pesticides. Although there have been several measurements of DDT concentrations in animal fat in different species, some doubt has been cast on the validity of the earlier measurements on the grounds that chromatographic techniques have only in recent years been able to distinguish unambiguously between DDT and its metabolic products such as DDE on the one hand and the chlorinated biphenyls on the other⁸. Since the late 1950s, there has been a great number of surveys of DDT concentrations in the body fat, as well as surveys of the prevalence of the isomers of DDT and its metabolic products. Even so, the information available, according to the Mrak Commission, is not sufficient to be sure that reported concentrations of DDT in the populations among which surveys have been carried out provide an accurate picture of the distribution of DDT in the body fat of human beings in the world as a whole. In the United States, a careful study of 734 samples of body fat collected during 1967 has revealed large variations in DDT concentrations from one person to another but has not so far made it possible to infer to what extent these variations are determined by such things as the variations of concentrations of DDT in the diet and constitutional variations. (In this connexion, the individual variations in the concentrations of dieldrin which have been revealed by the surveys carried out are much less.) For the predominant groups in the United States population (represented by 457 of the samples), DDT concentrations ranged between zero and 25.47 parts per million in fatty tissue, corresponding to a mean concentration of 4.17 parts per million and a median concentration of 3.32 parts per million. These and

other more extensive surveys have, however, shown that there are significant variations. There is, for example, some evidence, both from the United States and from Britain, that DDT concentrations are higher in males than females, but this is not considered significant. In the United States, DDT concentrations appear to be greater among non-whites than whites (see Fig. 1) but it is not known whether these differences are constitutional or environmental, a consequence of differences of dietary habits. Unlike the other persistent pesticides, concentrations of DDT seem also to increase with age, although in the United States this correlation is less marked in northern states than in the south. There appears to be a positive correlation of DDT with average temperature. Although the simplest interpretation of the positive correlation between DDT concentrations and age would suggest that the concentrations so far observed are not the product of an equilibrium condition in which dietary intake is balanced by excretion and metabolism, the Mrak Commission says that the lack of measurements on the body fat of younger children may invalidate this interpretation. Attempts to compare the results of surveys carried out at different times have so far been complicated by the wide scatter of the measurements, which is why all those responsible for these interpretations have urged the need for more extensive sampling.

Outside the United States, similar DDT concentrations have been measured. Measurements from Delhi, for example, suggest that DDT concentrations in fat in at least some parts of the population of India may be on the average higher than in the United States. Perhaps the most striking set of observations is that data for Alaskan Eskimos suggest concentrations of DDT and DDE amounting in sum to 5.2 parts per million even though the population concerned ate a diet containing negligible amounts of pesticides. Although there seems to be no doubt that food is the principal source of residues of DDT, this observation has raised the possibility that intake by drinking water or even air may be an important source of contamination—a hypothesis that will be more easily tested when methods for the analysis of DDT in air samples have been developed.

The question arises of what the biological consequences may be of DDT concentrations in human fat. It appears now to be established, on the basis of experiments in which individuals have been fed diets containing large quantities of the pesticides, that an equilibrium concentration of pesticide residue is reached after a period of the order of a year. The ultimate concentration of DDT appears to be proportional to dosage. The leaching of DDT from fatty tissue after exposure to abnormally large controlled doses of DDT appears to be pharmacokinetically similar to the uptake process, and this has been confirmed by studies of pesticide concentrations in the blood of workmen removed from industrial exposure in agriculture and in manufacturing plants. One study shows that concentrations were reduced by a factor of five after the ending of occupational exposure. The medical evidence of the consequences of DDT exposure has been authoritatively reviewed by the Mrak Commission. There is, of course, no doubt that in sufficiently large doses, DDT is an acute poison. For laboratory rats, the semi-lethal acute dose by the oral route is thought to be 113 mg kg⁻¹, which would imply, if this measurement were applicable to human beings, that something of the order of 10 g of DDT in a single dose would kill half of those to whom it was administered. In practice, on the basis of epidemiological studies, human beings are more resistant than rats but nevertheless pesticides such as DDT have been responsible for large numbers of accidental deaths in the United States and elsewhere since the Second World War. The use of pesticides in suicide is commonplace in advanced societies. Children are especially susceptible to accidental poisoning. Elsewhere there are horrendous accounts of how organochlorine pesticides contaminating food have killed large

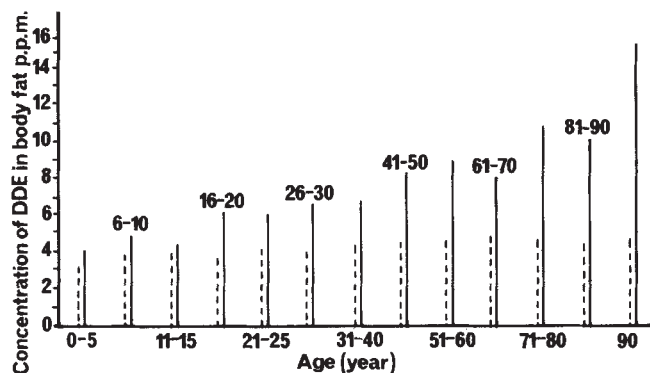


Fig. 1 Concentration of DDE in human body fat in the United States (1968). ---, Whites; —, non-whites.

numbers of people—for example, 102 people died in India in the mid-1950s after eating contaminated flour.

Attempts to demonstrate significant differences between the mortality and morbidity of agricultural workers using persistent pesticides in the general population have been suggestive but not conclusive. Among workers in chemical plants manufacturing pesticides, the Mrak Commission says that there appear to have been no detectable signs of ill-effects, even among workers whose body fat contained between 38 and 647 parts per million of DDT, corresponding to 10 per cent or more of the semi-lethal acute dose, although other members of the organochlorine family (lindane) are known to be more damaging. The Mrak Commission says that the "data supported the concept that the hazards of working with pesticides were slight" but emphasizes that statistics so far available leave much to be desired.

From the environmental point of view, however, the accumulation of much smaller concentrations of DDT in human body fat is the central issue, for this is how the population as a whole might be affected. The Mrak Commission says that the literature then available (at the end of 1969) suggested that "the only certain demonstrable effect of this exposure in man is the tissue residue" but it says that there are "obvious adverse ecological effects" among predator birds and fish, uncertainty about enzyme induction by DDT and anxiety about the findings of the Bionetics report on the carcinogenic potential of DDT in mice. One obvious difficulty is, of course, that attempts to identify the consequences of pesticide residues in fatty tissues are bound to be frustrated both by the low incidence of the effects to be expected and by the lack of an understanding of the variations of DDT concentrations from one individual to another.

Pesticides, like other synthetic chemicals, the natural carcinogen hydrocarbons included, are known to stimulate enzyme activity. One practical consequence of such effects might be to alter the susceptibility of an organism to drugs and other chemicals which are normally metabolized in the liver, and by now the capacity of the chlorinated hydrocarbons to increase enzyme activity in the liver has been well established. Although the Mrak Commission says that studies of enzyme induction should be a part of all future attempts to evaluate the safety of these and related chemicals, it concludes that "where quantitative data are available for the dose-response relationship for enzyme induction and for the dietary intake of pesticides, it is concluded that the present intake is insufficient to cause interaction resulting from enzyme induction. For DDT, sufficient information is available to permit this conclusion with reasonable certainty unless man responds to a greater extent than experimental animals".

The issue of carcinogenicity is harder to settle, chiefly because of the confusion generated by the Bionetics report—a broad survey of the carcinogenic potential of several synthetic chemicals, the pesticides included, which was commissioned in the mid-1960s by the National Cancer Institute

in the United States and published in mid-1969. The panel set up by the Mrak Commission to assess the carcinogenic hazards of pesticides considers that "this study has demonstrated that DDT increased the incidence of cancer in mice under the experimental conditions employed. However, this does not prove carcinogenicity for human beings at the very much lower level to which they are actually exposed". As a result, it asks that there should be more prolonged studies of the carcinogenic effects of low doses of pesticides. The Mrak Commission panel was not unanimous in its assessment of the value of the Bionetics study—the majority of the members acknowledged that the study had been devised in such a way, with the use of strains of mice in which spontaneous cancer is common, as to maximize the likely consequences of potentially carcinogenic chemicals; but other members of the panel argued that the doses of chemicals used were unrealistically high, that the feeding of test substances by means of stomach tubes in the pre-weaning period was unrealistic and that the methodology of the Bionetics study was improper in that whole litters of mice were treated either as experimental subjects or control animals and not divided as they should have been. On these grounds, the minority argument goes, the Bionetics study may have been unfairly biased.

These conclusions have since been confirmed by a study with experimental animals carried out at the International Agency for Research on Cancer at Lyons, where a study with low doses of pesticides has demonstrated the occurrence of hepatomas but not of malignant tumours in the ordinary sense among mice fed pesticides in their diet⁹. To be sure, there are bound to be serious difficulties in extrapolating from results with experimental animals to the likely behaviour of human beings in response to persistent chemicals in body fat but at least it now appears that Miss Rachel Carson's forebodings about the possibly high incidence of cancer as a result of the use of persistent pesticides have been undermined.

If there are no substantial reasons for fearing that people will be seriously affected by pesticide concentrations of the kind now present in the environment, the same is not true of other living species. For by the late 1950s, it had become apparent that pesticides could damage predatory birds and marine organisms either directly or by interfering with reproduction. This was the spirit in which the British government in the late 1950s introduced the still current regulations for the use of aldrin and the other more toxic relatives of DDT in dressing seed. Although the immediate alarm was that predatory birds and even woodpigeons were poisoned by eating the corn, it is now apparent that the potentially more serious (in the sense of being less controllable) consequences of the widespread use of DDT stem from the way in which the residues which accumulate in various species of birds, especially those which live off fish, can affect the survival of the species concerned, most obviously by reducing the thickness of eggshells and thus the viability of newborn chicks. Comparisons between museum specimens and eggs laid since the introduction of persistent pesticides¹⁰ show that this effect is indeed new, and the same effects have been demonstrated in the laboratory with mallard ducks and with American kestrels, where diets containing DDT, especially in combination with dieldrin, have been shown to reduce the rate of hatching of young.

Perhaps the most dramatic consequences for birds of small amounts of persistent pesticides are those recently observed among a colony of brown pelicans on Anacapa Island off the coast of southern California, where in 1969 only four brown pelicans hatched out of 1,200 nesting couples. Similar observations have been made with cormorants in southern California and brown pelicans off the Atlantic and Gulf coasts. According to the National Academy of Sciences committee on the subject⁶, "accumulations of chlorinated hydrocarbons in the southern California marine ecosystem

passed a level critical to the brown pelican". These are biological effects no longer in dispute. What does this imply for the management of persistent pesticides? Much depends on making some estimate of the degree to which the effects which have been observed off the coast of the United States will be replicated elsewhere and of likely accumulations of persistent pesticides in these species as time goes on. Although species such as human beings living off diets which are largely terrestrial in origin may have reached some kind of equilibrium, there is at least a possibility that the oceans of the world might become a repository of stocks of DDT—a hypothesis lent some force by the discovery of DDT in the livers and body fat of penguins and sea birds in the Antarctic. It is therefore relevant that DDT residues in Antarctic wild life are measured in parts per trillion, not parts per million, which shows that the spread of persistent chemicals is not nearly as uniform as it might seem to be, at least for the time being. The question of what happens to DDT that has been applied to agricultural land has been examined by the committee of the National Academy of Sciences⁶, which concluded that as much as 25 per cent of the persistent pesticides so far produced, the best part of half a million tons, might already have been washed into the sea by rivers or carried there by wind and rainfall. In reality, river transport into the oceans accounts for less than 3 per cent of annual production and perhaps only a tenth as much, but the committee responsible for the report estimated that the total transfer to the oceans of persistent pesticides by means of rainfall would be far greater. Indeed, the committee estimated that rainfall might be the chief source of oceanic DDT and that something like two-thirds of the annual production of persistent pesticides might eventually find its way into the oceans by means of rainfall. This calculation, however, was acknowledged at the outset to be approximate and is especially suspect because it was based on a single series of measurements of DDT concentrations in rainfall near Harwell in England carried out in 1967. There is in any case doubt about the fate of DDT in sea water, which has not so far been detected there by analytical methods. Do the persistent pesticides concentrate in the surface film where organic materials of various kinds are found, or is DDT degraded rapidly in sea water as at least one set of experiments has suggested¹¹?

Until these questions are settled, it will not be possible to foretell the extent to which the troubles which have afflicted brown pelicans off the coast of North America will be replicated elsewhere. If it turns out that the oceans are not effective as a means of spreading DDT throughout the ecosystem and in particular of contaminating important food chains at their base, it will be reasonable to expect that the difficulties which sea birds in North America have experienced will be confined to regions where persistent pesticides are used intensively and that the damage done will be reduced as the use of the persistent pesticides diminishes as a result of tighter regulations.

Even if the most gloomy interpretation of the data now available turns out to be correct, the result will not be a threat of worldwide catastrophe for the human race but rather an economic question of an unusual kind—how is it possible to balance against each other the damage done to particular species of animals by the use of persistent pesticides and the benefits of their past and future use? There are the strongest possible reasons for preserving as many living species as possible, if only for aesthetic and historical reasons. There is, however, no reason to think that the damage done to the populations of wild birds by pesticides is as yet comparable to the damage done by hunting, thought to be responsible for the deaths of 2 million game birds a year in North America alone, and the economic benefits of persistent pesticides in agriculture and in public health hardly need restating. In short, the persistent pesticides seem to be not so much a threat to the survival of the human

race as a means for its survival with unpleasant and sometimes unexpected side effects which need hard-headed calculation¹².

In the past few years, other quite different mechanisms have been considered as possible causes of the contamination or transformation of the worldwide environment. In the nature of things, each mechanism is necessarily complicated, and usually more complicated than first appears. By now, perhaps the most familiar of these mechanisms is the so-called greenhouse effect, the method by which the accumulation of carbon dioxide in the upper atmosphere might affect the climate on the surface of the Earth and in particular increase the temperature as a consequence of the increased opacity of carbon dioxide to infrared radiation. At times, the greenhouse effect has been considered seriously enough to feature in declarations on the environment by, for example, the President of the United States. A part of the difficulty is that of knowing precisely what is the trend of carbon dioxide concentrations which fluctuate by 2.5 per cent from winter to summer and which have in any case been measured systematically at only a few observatories, principally at Mauna Loa (Hawaii). It does, however, appear that the average concentration, at present 322 parts per million (by volume), is increasing at one part per million per year or by about four per cent per year, which implies that roughly half the carbon dioxide emitted by power stations and other terrestrial sources will remain in the atmosphere. (The rest is transferred to the oceanic reservoir.) After 1980, the growth rate is expected to be 3.5 per cent a year, with the increasing introduction of nuclear power stations.

The climatic consequences of these increasing concentrations have been much harder to predict. The most reliable calculations¹³ suggest that the predicted concentration of 375 parts per million by the end of this century would entail an increase at the surface of 0.5° C (with an accompanying decrease of temperature in the stratosphere), but the model used in this calculation makes no allowance for geographical variations. Moreover, it now appears that the predicted increase of temperature of 0.5° C would be cancelled out by a 0.6 increase of cloud cover over the surface of the Earth, not merely a small variation by meteorological standards but also the kind of variation likely to be stimulated by an increase of surface temperature. In other words, the more pessimistic calculations of the consequences of the increase of carbon dioxide in the atmosphere have ignored—for perfectly understandable reasons—a negative feedback mechanism which may imply that surface temperatures are insensitive to the concentration of carbon dioxide in the atmosphere.

¹ Carson, R., *Silent Spring* (Alfred A. Knopf, 1962).

² *Final Environmental Impact Statement on Proposed Trans-Alaska Pipeline* (National Technical Information Service, US Dept of Commerce).

³ *First Report of Royal Commission on Environmental Pollution* (HMSO, 1971).

⁴ *Environmental Quality* (Annual Report of Council on Environmental Quality, 1971).

⁵ Peterle, T. J., *Nature*, **224**, 620 (1969).

⁶ *Chlorinated Hydrocarbons in the Marine Environment* (US National Academy of Sciences, 1971).

⁷ *Report of the Secretary's Commission on Pesticides and Their Relationship to Environmental Health* (Mrak Commission) (US Dept of Health, Education and Welfare, 1969).

⁸ Richardson, A., *Organochlorine Compounds other than Insecticides in the Environment* (Shell Research Ltd, 1969).

⁹ *Annual Report, 1970* (WHO International Agency for Research on Cancer, 1971).

¹⁰ Ratcliffe, D. A., *Nature*, **215**, 208 (1967).

¹¹ Wilson, jun., A. J., Forester, J., and Knight, J., *Chemical Assays* (US Dept of the Interior, Circular 335, August 1970; Gulf Breeze Laboratory, Florida, Progress Report FV 1969).

¹² Headley, J. C., and Lewis, J. N., *The Pesticide Problem: An Economic Approach to Public Policy* (Resources for the Future, Inc., 1967).

¹³ Manabe, S., and Wetherald, R. T., *Thermal Equilibrium of the Atmosphere with a Given Distribution of Relative Humidity* (*J. Atmos. Sci.*, **24**, 241; 1967).

Significance of Molecular Alignment and Orbital Steering in Mechanisms for Enzymatic Catalysis

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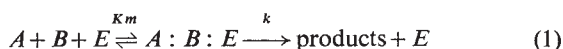
Calculations of the energy required to form misaligned transition states in the confines of a solvent cage indicate that many reactions could be very considerably catalysed by precise alignment (orbital steering) in enzyme active sites.

Storm and Koshland¹ pointed out that enzymatic catalysis of chemical reactions is generally far greater than would be expected from the proximity of the reactants and the catalytic groups of the enzyme in the active site. They attribute this to precise alignment of reactants and catalytic groups in the active site, ensuring the formation of a correctly aligned transition state for the reaction; they call this "orbital steering". They also cite data on the non-enzymatic lactonization rates of several hydroxy-acids, which exceed the rates of bimolecular esterification reactions by a factor larger than they predict from a simple "proximity effect". They attribute this reactivity to a similar intramolecular alignment in a non-enzymatic system.

Bruice *et al.*², however, consider that an enzyme cannot achieve significant catalysis through orbital steering unless the normal uncatalysed reaction requires the alignment of the reacting molecules to within a fraction of a degree. They show that this is unreasonable on energetic grounds, and conclude that there is no evidence for orbital steering. I wish to consider the concepts of misalignment in a chemical reaction in more detail, and show that there is considerable scope for an enzyme to catalyse a reaction through orbital steering.

Orbital Steering and Enzyme Catalysis

Many enzymes seem to conform to the Michaelis-Menten mechanism shown in equation (1) (*A* and *B* are reactants, *E* is the enzyme, and the second step is rate determining)



while the corresponding uncatalysed reactions of *A* and *B* are kinetically second order. Direct comparison of the rate constants is therefore not possible. However, a helpful comparison can be made on the basis of the following model of bimolecular reactions.

Simple bimolecular reactions in solution occur in two stages. The two reactants form a molecule pair in which the reacting atoms in each molecule occupy adjacent lattice sites in the solvent. The molecule pair then forms a transition state involving the overlap of the reacting orbitals, which can decompose to yield the products of the reaction, as in equation (2).



The rate constants *k* and *k_c* are now directly comparable and give the ratio of the average rate of reaction of a molecule pair in the enzyme active site to the average rate of reaction of a

molecule pair in free solution. *k* is generally greater than *k_c*, and it has been suggested that this could be caused, in the active site, by the presence of catalytic groups or electrostatic fields, by the production of steric strains in or between the reactants, or by accurate alignment of the reactants. Jencks³, however, concluded that alignment is not important. It is the contribution of alignment (known as orbital steering by Storm and Koshland¹) which I wish to debate, placing particular emphasis on the effect of the solvent cage around the reacting molecules. The average reaction rate of molecule pairs in solution is compared with the reaction rate of ideally aligned molecule pairs. This ratio is referred to as the "alignment factor". The reciprocal of the alignment factor gives the maximum catalytic effect which an enzyme can achieve by ensuring that all molecule pairs in the active site are ideally aligned.

Misaligned Molecule Pairs and Bent Transition States

The energy of the transition state in simple bimolecular reactions will depend on the mutual alignment of the reactants in the molecule pair. Fig. 1*a* shows the most favourable situation in which the axes of the reacting orbitals coincide with the line between the nuclei of the reacting atoms, *A'B'*. Fig. 1*b* shows a planar bent transition state; this might be produced from a molecule pair in which the axes of both reacting orbitals are misaligned from the internuclear axis, but still lie in the same plane. In this case, where $\alpha \approx \beta$, a reasonable orbital overlap is still achieved, and the energy of the transition state exceeds that of the ideally aligned transition state approximately by the energy required to bend the aligned transition state to the bent conformation. Figs. 1*c* and *d* show situations in which the misalignment of the molecule pair has prevented significant orbital overlap, so that neither stable transition states nor products can be formed.

A good orbital overlap is essential to the formation of a low energy transition state, and therefore two reacting molecules will tend, as they approach the transition state, to achieve maximum orbital overlap by slight sideways adjustments of

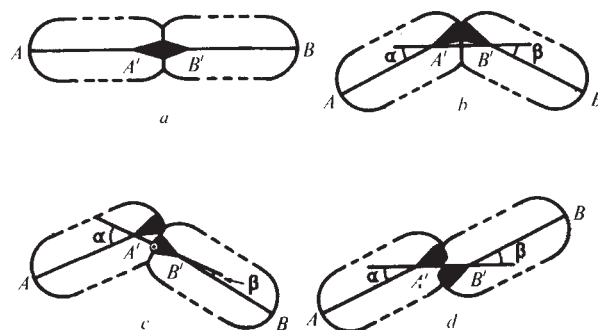


Fig. 1 Various types of planar aligned and misaligned transition states. These diagrams illustrate how misalignment of the reactants can produce bent bonds and poor orbital overlap in the transition state. In these cases the axes of the reacting orbitals, *AA'* and *BB'*, are assumed to be coplanar.

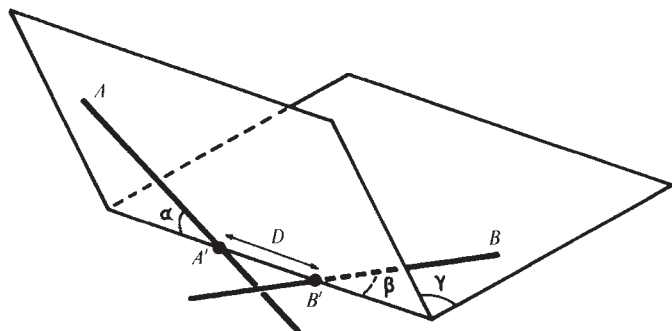


Fig. 2 A non-planar misaligned transition state. A' and B' are the nuclei of the reacting atoms, and D is the internuclear distance in the transition state. AA' and BB' are the axes of the reacting orbitals, inclined at angles α and β respectively to the internuclear axis, $A'B'$. The two axes need not lie in the same plane, and γ is the dihedral angle between the planes $AA'B'$ and $A'B'B$.

their positions. Thus, Fig. 1c could be converted to Fig. 1b by a small downward displacement of A' and a similar upward displacement of B' . If the magnitude of these displacements were small compared with the size of the reacting molecules, the total degree of misalignment, $\alpha + \beta$, is not significantly altered, but the stability of the transition state is greatly increased by the more effective orbital overlap which is achieved. Energy is required to displace A' and B' from their positions of minimum potential energy within the solvent cage, and this will contribute to the activation energy. I assume that the energy gain from good orbital overlap considerably exceeds the corresponding energy required to displace the molecules within the solvent cage, and therefore that the molecules will react so as to achieve planar symmetrically misaligned transition states such as that in Fig. 1b, where $\alpha = \beta$.

In reality, the misalignment of molecule pairs occurs in three dimensions, and the degree of misalignment must be expressed by three angles, α , β and γ (Fig. 2). Comparison of Figs. 1 and 2 shows that better orbital overlap can still be achieved in the non-planar case by small displacements of A' and B' . The best orbital overlap will be obtained by displacements which make α and β approximately equal, and the orbital axes AA' and BB' coplanar.

In a non-planar molecule pair, the misalignment of the transition state is no longer given simply by the sum of the initial values of α and β , but is also a function of γ . The final value of α , in the transition state (assumed equal to β), and the size of the displacements, d , of A' and B' can be calculated for any molecule pair from the internuclear distance in the transition state, D , and the initial values of α , β and γ in the molecule pair.

Using Fig. 6, and standard equations in spherical trigonometry, one can show that the final total misalignment of the transition state ($2 \times \widehat{AOD}$) can be calculated from $\cos(2 \times \widehat{AOD}) = \cos \alpha \cos \beta - \sin \alpha \sin \beta \cos \gamma$, and that the magnitude of the displacement of each of the reacting atoms from their equilibrium positions in their solvent cages will be $D \sin(\widehat{COD}/2)$ (where

$$\cos \widehat{COD} = \cos \alpha \sqrt{(1 - \cos \widehat{AOB})/2} + \frac{\cos \alpha \cdot \cos \widehat{AOB} + \cos \beta}{\sin \widehat{AOB}} \sqrt{(1 + \cos \widehat{AOB})/2} \text{ and}$$

$$\cos \widehat{AOB} = -\cos \alpha \cdot \cos \beta + \sin \alpha \cdot \sin \beta \cdot \cos \gamma$$

Calculation of Activation Energies

I have suggested that two factors make the activation energy for a misaligned molecule pair higher than that for an ideally aligned pair. These are the bond strain energy in a transition state in which the partially formed bond is bent, and the energy required to displace the reactants in the solvent cage to achieve good orbital overlap. These energies can be calculated from the

total misalignment of the transition state (equal to 2α) and the size of the displacements in the solvent cage.

The bond strain energy can be calculated from equation (3), where Q is the bending constant of the bonds⁴

$$E^* (\text{bond strain}) = Q \cdot (2\alpha)^2 / 2 \quad (3)$$

The displacement energy can be estimated from a simple model of the solvent cage shown in Fig. 3 (taking $d_a = d_e = 0$). A small displacement, d , of the central atom along AB brings it closer to three of the axial solvent molecules by $\sqrt{2/3} \cdot d$, and further from the other three by an equal distance. The distance of the central atom from the equatorial atoms changes negligibly. The displacement energy can therefore be calculated from equation (4), where d_{eq} is the sum of the van der Waals radii for the molecules involved. This equation is based on the Lennard-Jones "6-12" equation (reviewed by Scott and Scheraga⁵).

$$E^* (\text{displacement}) = 3k_1 / (d_{eq} - \sqrt{2/3} \cdot d)^6 + 3k_2 / (d_{eq} - \sqrt{2/3} \cdot d)^{12} \\ + 3k_1 / (d_{eq} + \sqrt{2/3} \cdot d)^6 + 3k_2 / (d_{eq} + \sqrt{2/3} \cdot d)^{12} \\ - 6k_1 / d_{eq}^6 - 6k_2 / d_{eq}^{12} \quad (4)$$

The values assigned to the constants Q , k_1 and k_2 are clearly important and are discussed below.

The excess activation energy which any misaligned molecule must overcome, compared with an ideally aligned molecule, can therefore be calculated from equation (5).

$$E^* = E^* (\text{bond strain}) + E^* (\text{displacement}) \quad (5)$$

Effect of Misalignment on Overall Reaction Rates

The effect of misalignment of molecule pairs on the overall rate of a reaction can be calculated by comparing the rate of a reaction in which the molecule pairs are randomly distributed among all possible degrees of misalignment with the rate of an ideal reaction in which all molecule pairs are ideally aligned. This ratio is referred to below as F , the alignment factor, and the reciprocal of F is equal to the maximum factor by which an enzyme can catalyse the reaction by orbital steering.

The ratio between the reaction rate constant for an ideally aligned molecule pair (k_{ideal}), and that for a particular type of

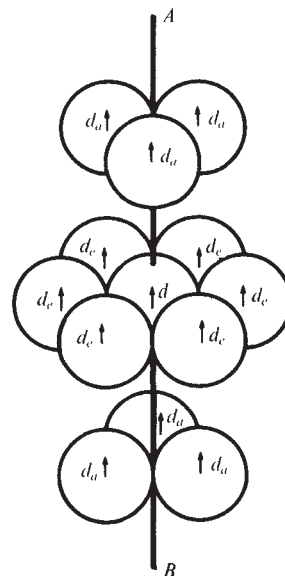


Fig. 3 Elastic distortion of the first solvation shell. The central atom is surrounded by twelve molecules in the first solvation shell, comprising two groups of three axial molecules (shown expanded along the three-fold axis AB for clarity) and six equatorial molecules. If the central atom is displaced by d along AB , then the axial and equatorial atoms may also move, by d_a and d_e respectively, in a parallel direction to new positions of minimum potential energy.

misaligned pair defined by a set of initial values for α , β and γ (k_{mis}), can be calculated from the difference between their activation energies, using the Arrhenius equation 6 (c.f. equation (5) for E^*).

$$k_{mis}/k_{ideal} = e^{-E^*/RT} \quad (6)$$

The alignment factor can therefore be obtained from a suitably weighted integration of this ratio over all types of misaligned pairs corresponding to all combinations of initial values of α , β and γ .

Referring to Fig. 2, it is assumed that all possible orientations in space of the axes of the reacting orbitals AA' and BB' are equally probable. It is then easy to show that all A' molecules for which α lies in the range α' to $\alpha' + d\alpha$ are orientated within a solid angle $2\pi \sin \alpha' d\alpha$, and therefore constitute a fraction $0.5 \sin \alpha' d\alpha$ of all A' molecules. Similar consideration of B' molecules yields an analogous factor $0.5 \sin \beta' d\beta$. It is also evident that the proportion of molecule pairs for which γ lies in the range γ' to $\gamma' + d\gamma$ is simply $d\gamma/2\pi$. The appropriate statistical weighting factor in the integration for a molecule pair defined by α' , β' and γ' is therefore $\sin \alpha' \sin \beta' / 8\pi$ and the alignment factor can be calculated from equation (7).

$$F = \frac{1}{8\pi} \int_0^\pi \int_0^\pi \int_0^{2\pi} \sin \alpha \sin \beta e^{-E^*/RT} \cdot d\alpha d\beta d\gamma \quad (7)$$

This integral was evaluated by a simple numerical procedure using an ALGOL program and an Elliot 4130 computer. The necessary values of E^* were calculated from equation (5). The internuclear distance, D , in the transition state was assumed to be 2.1 Å. Ranges of values were tried for the constants Q , k_1 and k_2 .

Fig. 4 shows the extent to which misaligned molecule pairs contribute to the total reaction rate. It is evident that a significant fraction of the reaction occurs through molecule pairs with a considerable degree of misalignment.

Value of Alignment Factor

The relationship between the value of the alignment factor, F , and the values of the constants Q , k_1 and k_2 is shown in Table 1.

Q is the bending constant of the partially formed bond in the transition state. Bending constants of this type derived from infrared spectroscopy are typically around 330 kJ/mol/radian⁴, but the bending constant for a partially formed bond in a transition state would naturally be smaller than this. A range of possible values down to 33 kJ/mol/radian have been con-

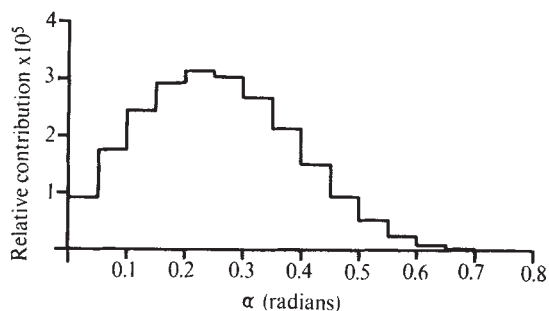


Fig. 4 The contribution of misaligned molecule pairs to the total reaction rate. The blocks of the histogram represent the contribution to the reaction rate of molecule pairs for which the misalignment of one atom, α , lies in the range specified by the abscissae. The ordinates give the ratio of this contribution to the total rate of the same reaction in which all the molecule pairs are ideally aligned (that is

$$\text{Relative contribution} = \frac{1}{8\pi} \int_0^\pi \int_0^\pi \int_0^{2\pi} \sin \alpha \sin \beta e^{-E^*/RT} \cdot d\alpha d\beta d\gamma$$

E^* is calculated from equations (3), (4) and (5), assuming $Q = 167$ kJ/mol/radian², $k_1 = -763$ kJ/mol/Å⁶ and $k_2 = 3.15 \times 10^5$ kJ/mol/Å¹².

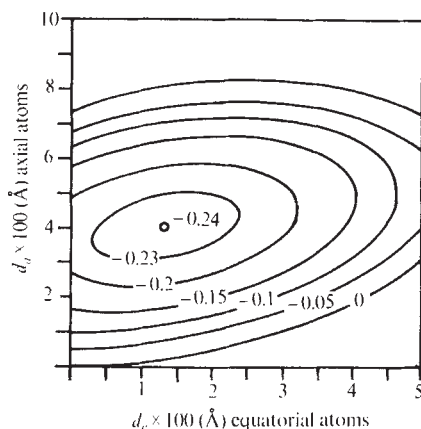


Fig. 5 The effect of elastic distortion of the first solvation shell on displacement energies. Referring to Fig. 3, the energy required to displace the central atom by 0.2 Å along AB is 1.1 kJ/mol (from equation (4) with $k_1 = -1,530$ kJ/mol/Å⁶ and $k_2 = 6.3 \times 10^5$ kJ/mol/Å¹²). The contours in this figure indicate the additional energy changes which accompany movement of the six axial and equatorial atoms by d_a and d_e respectively in a parallel direction. Energies are calculated from an expanded version of equation (4), using the same constants, and assuming that the surrounding solvent molecules in the second and further solvation shells do not move. An energy minimum of -0.24 kJ/mol exists corresponding to the new stable positions of the molecules in the solvent cage, and the decrease in energy reduces the energy required to displace the central atom by 22%.

sidered in Table 1, but it seems probable that Q for many transition states will lie in the range 84–167 kJ/mol/radian.

k_1 and k_2 describe the attractive and repulsive forces between the reacting molecules and the surrounding solvent cage (cf. equation (4)). For contact between atoms of van der Waals radii about 1.5 Å, k_1 and k_2 have values of about -1,530 kJ/mol/Å⁶ and 6.3×10^5 kJ/mol/Å¹² respectively⁵; smaller values of these constants are also considered in Table 1, ranging from as low as one tenth of the above values, but the ratio k_1/k_2 is maintained constant so that the van der Waals radii remain constant while the force constants are varied.

The half-life of a transition state is very short (equal to $h/kT \approx 2 \times 10^{-13}$ s at 273 K). During the formation and life of the transition state, the shape of the surrounding solvent cage may undergo some elastic distortion, since the time interval is long compared with normal vibrational frequencies. However, the lifetime of the transition state is too short for molecules of the solvent to reorganize their hydrogen bonding or shift to different lattice sites. In Figs. 3 and 5 displacement of a molecule by 0.2 Å in a rigid solvent cage requires 1.1 kJ/mol, and this is only reduced by 0.24 kJ/mol (22%) when the molecules in the first solvation shell are allowed to shift to their new positions of minimum potential energy. Although this is a simple model, the relatively small reduction in displacement energy arising from the elastic distortion of the first solvation shell suggests that elastic distortion of the second and further solvation shells would produce little further reduction in displacement energy. The true displacement energies can be approximated from equation (4) if the appropriate empirical values of k_1 and k_2 are adopted. Even with coordination

Table 1 Values of F , the Alignment Factor, calculated for Various Values of Q (the Bond Bending Constant of the Transition State) and k_1 *

k_1 (kJ/mol/Å ⁶)	Q (kJ/mol/radian ²) 334	167	83	33
-1,530	6.6×10^{-5}	1.3×10^{-4}	2.4×10^{-4}	5.9×10^{-4}
-763	1.2×10^{-4}	2.2×10^{-4}	4.3×10^{-4}	—
-382	2.0×10^{-4}	3.8×10^{-4}	7.2×10^{-4}	—
-153	3.7×10^{-4}	—	—	3.0×10^{-3}

* The constant involved in intermolecular attractions, compare equation (4). The value of k_2 required for equation (4) was calculated for each value of k_1 from $k_2/k_1 = -4.11 \times 10^2$ Å⁶.

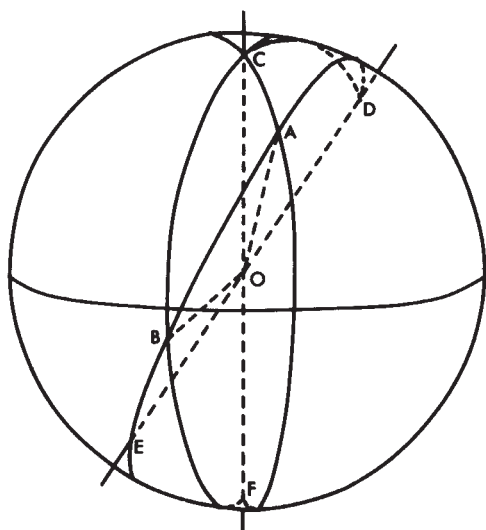


Fig. 6 Calculation of the displacements of atoms forming the transition state. The nuclei of the reacting atoms lie initially along the axis CF very close to O, and the axes of the reacting orbitals lie in the directions AO and BO respectively (compare with Fig. 2). The angles α , β and γ are equivalent to $\angle AOC$, $\angle BOF$ and $\angle ACB$ respectively. On attaining a planar, symmetrically misaligned transition state, the axis through the reacting atoms shifts to ED, so that $\angle AOD = \angle BOE$.

numbers around 9–10 these values are unlikely to lie below the range 25–50% of the values for a rigid solvent cage of twelve molecules.

Table 1 shows that, if Q lies in the range 84–167 kJ/mol/radian and k_1 and k_2 in the range 25–50% of the values for the rigid solvent cage, then the alignment factor lies in the range $2\text{--}7 \times 10^{-4}$. This range of values would allow an enzyme to achieve catalysis by a factor of $1.5\text{--}5 \times 10^3$ simply through orbital steering, without the participation of any catalytic groups in the enzyme active site (these values are derived directly from the reciprocals of the alignment factors).

Conclusions

These calculations suggest that enzymes can achieve a considerable degree of catalysis by precise alignment of reacting molecules and catalytic groups in the active site. The most conservative estimate for the alignment factor (7×10^{-4}) suggests that a reaction involving two reactants and two cata-

lytic groups could be accelerated by a factor of 2.9×10^9 through orbital steering. This factor is almost certainly larger than the degree of catalysis which the enzyme can achieve over the normal uncatalysed reaction. In free solution, a transition state involving two reactants and two separate catalyst molecules would be improbable at normal concentrations, and the reaction would probably proceed faster by a simpler mechanism involving fewer catalytic molecules which is therefore not strictly comparable with the transition state in the enzyme active site³.

Bruice *et al.*² criticize Storm and Koshland¹ for basing their arguments on a "naive" model of orientation effects, but they use virtually the same model themselves when they conclude that orbital steering can only be effective for reactions which require alignment of their reactants to within a fraction of a degree. The model discussed here considers the consequences of misalignment not only on bond energies in the transition state, but also on interactions with the surrounding solvent cage. The results bear out the conclusion of Bruice *et al.*² that molecule pairs with a considerable degree of misalignment can contribute significantly to the total reaction rate (compare Fig. 4). The solvent cage, however, imposes a considerable constraint on the reaction of certain types of misaligned molecules. Taking account of both bond energy and solvent cage effects, these calculations do not support the conclusion of Bruice *et al.* that there is no theoretical justification for the concepts of orbital steering. The catalytic factor of $1.5\text{--}5 \times 10^3$ derived above for orbital steering lies towards the bottom of the range of $10^3\text{--}10^5$ which Storm and Koshland suggest might reasonably account for the catalytic power of many enzymes. This estimate is based on rather conservative estimates of the molecular constants involved. Therefore, these results provide support for the concept of orbital steering, even though the molecular model used differs in several important respects from that of Storm and Koshland.

I thank Dr G. R. Penzer for constructive comments.

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Galactic Structure and the Apparent Size of Radio Sources

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Observations of radio sources at 81.5 MHz show that interstellar scattering is more important than some pulsar data suggest. Low frequency measurements also provide a new means for studying the distribution of ionized gas in the Galaxy.

THERE is now strong evidence that intensity variations of pulsars on time scales of minutes to hours are the result of interstellar scintillation^{1–7}. The effect is caused by the scattering of radiation by plasma density variations, which have a typical scale of $\sim 10^{11}$ cm and which modulate the ambient density by about 0.1% (ref. 4). The distances of most pulsars are still uncertain, but estimates of the total angular scattering which might occur along a line of sight through the Galaxy have been made^{2,4,8}. Such estimates are important because high resolution observations in radio astronomy may be severely limited by bad seeing in the Galaxy⁸.

In this article we describe new results, derived from a survey of radio sources at 81.5 MHz, which show that interstellar scattering is more important than some of the pulsar data might suggest. As well as providing a more realistic assessment of the resolution limit imposed by interstellar scintillation, our observations indicate that the measurement of radio source diameters at low frequencies provides a valuable new technique for studying the distribution of ionized gas within the Galaxy.

Observations

The observations were made with the 20,000 m² array at Cambridge which was designed for the study of radio sources of small diameter using the method of interplanetary scintillation. The instrument and technique have been described by Hewish and Burnell⁹. Radio source diameters in the range 0.1 to 1.0 arc s are determined by the method described by Readhead¹⁰. The entire sky above declination -10° has now been surveyed at regular intervals throughout a year and the data are being analysed.

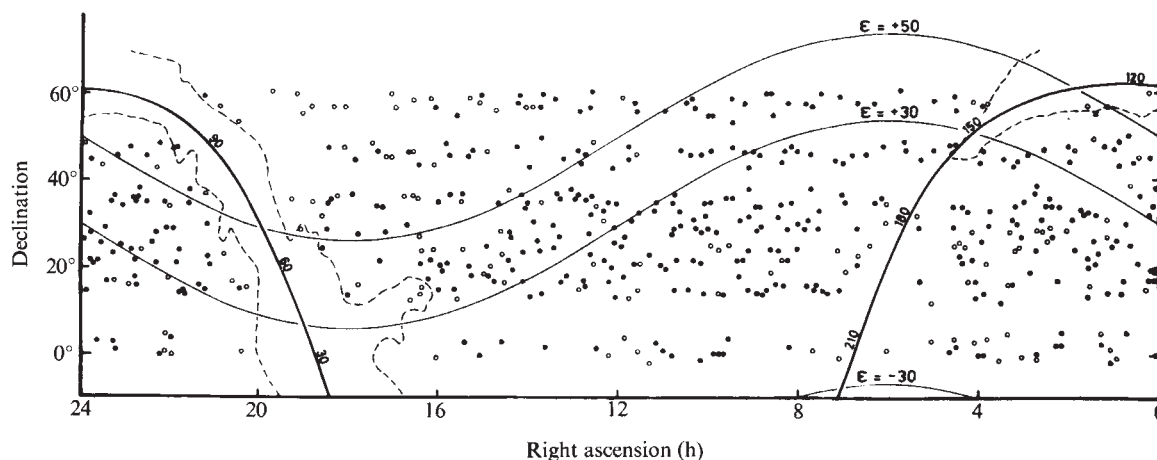


Fig. 1 The distribution of 4C sources observed to scintillate at 81.5 MHz. The bars on the right indicate declination strips for which the analysis is complete and curves corresponding to minimum solar elongation of $+50^\circ$ and $+30^\circ$ (above the ecliptic) and -30° (below the ecliptic) are shown. The dashed lines are 400 K contours of the galactic background at 178 MHz. ●, Scintillators; ○, probable scintillators.

The distribution of sources which exhibit interplanetary scintillation, and which are identified with sources in the 4C catalogue^{11,12}, is shown in Fig. 1. This plot indicates a significant absence of scintillating sources for lines of sight within roughly $\pm 15^\circ$ of the galactic plane, implying that in this region the apparent angular diameters of radio sources are considerably increased by interstellar scattering.

Is this distribution attributable to observational selection? Two factors which must be considered are the decrease of scintillation index with increasing solar elongation, and a reduction of threshold sensitivity for detecting scintillation, caused by increased system noise when the galactic plane is within the antenna beam.

We have found that at 81.5 MHz scintillation is greatest at a solar elongation, ϵ , of 30° , and falls to half this value for elongations larger than 50° . In Fig. 1 we have drawn lines corresponding to $\epsilon = 30^\circ$ and $\epsilon = 50^\circ$. All scintillating sources which lie below the curve for $\epsilon = 30^\circ$ will attain the maximum scintillation at some time during the year. The scintillation of an equivalent source lying above the curve for $\epsilon = 50^\circ$ will achieve a maximum of less than half this value. It is clear that the absence of scintillating sources at low galactic latitudes cannot be attributed to the effect of solar elongation, because it occurs where scintillation should be most pronounced.

We made measurements of the increase of system noise attributable to the Galaxy during routine calibrations of the survey, and derived the corresponding reduction of sensitivity. As we shall show, this effect cannot account for the lack of

scintillating sources at low galactic latitudes. We therefore believe that this effect is caused by apparent broadening of sources by the interstellar medium.

Interstellar Scattering

In order to explain the absence of scintillating sources at low galactic latitudes the interstellar scattering must be $\gtrsim 1$ arc s at 81.5 MHz. It might be expected that an increase of source diameters would be observed as the cutoff region is approached, and that this would enable the interstellar scattering to be measured directly. Unfortunately the cutoff is abrupt, so the number of sources in the present sample is small. This factor, combined with a large scatter of intrinsic source diameters, makes direct measurement inaccurate. We have adopted a statistical method to overcome this difficulty.

If a fraction α of the detectable sources at high galactic latitudes is observed to scintillate, the expected fraction of scintillating sources at lower latitudes is $\alpha\beta^x$, where β is the fractional decrease of sensitivity relative to high latitudes,

caused by increased system noise, and x is the slope of the number-flux density relation for the radio sources. Because the 81.5 MHz survey has a sensitivity comparable with 4C we put $x = 1.67$ (ref. 13), and β was calculated from the measured system noise and the known distribution of galactic radiation¹⁴.

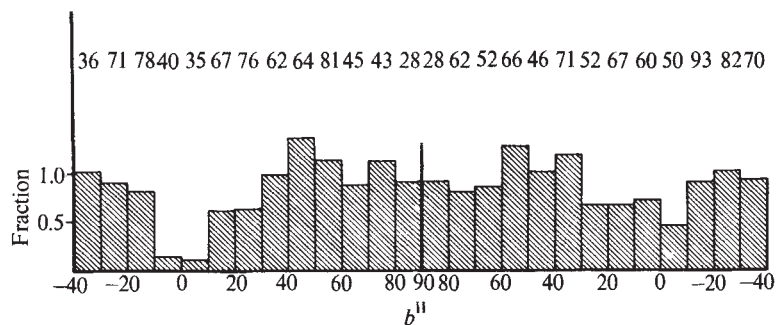
The observed deficit of scintillating sources, corrected for the factor $\beta^{1.67}$ and expressed as a fraction of the number of scintillating sources observed at high galactic latitude, is shown in Fig. 2. In computing these values intervals of 10° were taken in galactic latitude b^{II} and the anti-centre region was considered separately from the longitude range $40^\circ < l^{\text{II}} < 120^\circ$. The number of 4C sources which could have been observed to scintillate, in the absence of interstellar scattering, is also shown for each latitude interval. It is clear from Fig. 2 that there is a pronounced deficit of scintillating sources in the range $40^\circ < l^{\text{II}} < 120^\circ$ whereas the deficit is barely significant in the direction of the anti-centre.

In order to estimate the magnitude of the interstellar scattering we now attempt to find a simple model to account for the observations. If θ_s is the angular scattering produced by the interstellar gas and θ_i the intrinsic diameter of an assumed gaussian source, the apparent angular diameter is given by $\theta_A = (\theta_i^2 + \theta_s^2)^{1/2}$. If the distribution of scattering irregularities is stratified, so that it depends only on distance above the galactic plane, we have also

$$\theta_s = \theta_0 (\sin b^{\text{II}})^{-1/2} \quad (1)$$

where θ_0 is some constant.

Fig. 2 The relative fraction of scintillating sources at different galactic latitudes. The fraction has been normalized to unity for latitudes $b'' > 40^\circ$ where roughly 40% of the 4C sources scintillate. The left half of the plot is taken from data in the range $24 \text{ h} > \alpha > 13 \text{ h}$ of Fig. 1, the right half from data in the range $13 \text{ h} > \alpha > 0 \text{ h}$. The total number of detectable 4C sources in each latitude range is shown at the top of the figure.



The extent to which θ_s reduces the fraction of scintillating sources is given by the factor

$$\beta' = [F(\theta_s)/F(\theta_0)]^{1.67} \quad (2)$$

where $F(\theta)$ is the maximum scintillation index which would be observed for a source of diameter θ . The value of $F(\theta)$ has been determined previously (see Fig. 8 of ref. 10). The assumption of non-gaussian source models will have only a small effect on β' as may be seen from Fig. 2 of Little and Hewish¹⁵. If θ_0 were known it would now be possible to compute β' for varying values of θ until a fit to the observations was obtained. Unfortunately, θ_0 cannot be determined independently of θ_s .

Fig. 3 shows the distribution of angular diameters for the sources we observed at high galactic latitudes with mean scintillating fluxes of at least five times the system noise. The diameters have been determined by the method described by Readhead¹⁰ and we believe they are accurate to ± 0.1 arc s. In order to derive the expected numbers of scintillating 4C sources at different flux levels we need to know how the distribution shown in Fig. 3 changes with the mean scintillating flux. The statistics do not allow this at present and we therefore assume that this distribution is independent of flux density.

It is instructive to suppose, initially, that $\theta_0 \ll \theta_s$ which would be the case if apparent source diameters at 81.5 MHz were entirely due to interstellar scattering. The values of Fig. 3 suggest $\theta_0 \sim 0.5$ arc s as a mean value. Adopting $\theta_0 = 0.5$ arc s and using relations (1) and (2) it is now possible to compute the deficit of scintillating sources at low latitude. A comparison of this with the observations (Fig. 4) indicates that $\theta_0 = 0.5$ arc s gives a significantly larger deficit than is actually present for $b'' > 15^\circ$, although for $b'' < 10^\circ$ approximate agreement is obtained. Confining our attention, for the present, to $b'' > 15^\circ$ it follows that $\theta_0 < 0.5$ arc s.

As another extreme we assume $\theta_0 = 0.1$ arc s. In this case the distribution of θ_s must be very similar to the observed values although a small adjustment (about 10%) is required to increase the relative number of sources in the range 0 to 0.2 arc s. Calculations with this model (Fig. 4) underestimate the deficit. So $0.1 \text{ arc s} < \theta_0 < 0.5 \text{ arc s}$. Calculations for $\theta_0 = 0.2$ arc s and 0.3 arc s are also shown, and indicate a reasonable fit to the observations for $0.2 \text{ arc s} < \theta_0 < 0.3 \text{ arc s}$ in the latitude range $b'' > 15^\circ$.

Deviations from the ideal stratified model are, of course, to be expected and we have already seen that lines of sight for $b'' < 10^\circ$ give roughly twice this predicted scattering. Fig. 1 gives some evidence for enhanced scattering near the north galactic spur. Again, the marked decrease of scattering at longitudes approaching the anti-centre may be explained by the smaller number of H II regions beyond 8 kpc from the galactic centre¹⁶. The available data are insufficient to allow a more detailed study of correlations between the distribution of scintillating sources and other galactic features, but we hope this will be possible once the data analysis is complete.

The effect of the Galaxy on the distribution of scintillating sources shown in Fig. 1 is very marked. It is therefore interesting to compare these results with the survey of scintillating sources made by Cohen, Gundermann and Harris¹⁷. Their

data, at 195 MHz, show some indication of a galactic latitude dependence similar to ours, but the extent to which this arises from the initial selection of sources is not entirely clear.

Comparison with Pulsar Data

Measurements of θ_s give no information about the integrated interstellar density if the modulation caused by the irregularities is small. In principle, however, it is possible to calibrate our values of θ_s by comparison with independent pulsar scintillation and dispersion measurements. If the ratio $\langle \Delta N_e^2 \rangle / \langle N_e \rangle$ is assumed to be constant, the integrated electron content along a line of sight which extends beyond the Galaxy may then be found if $\langle N_e \rangle$ is known.

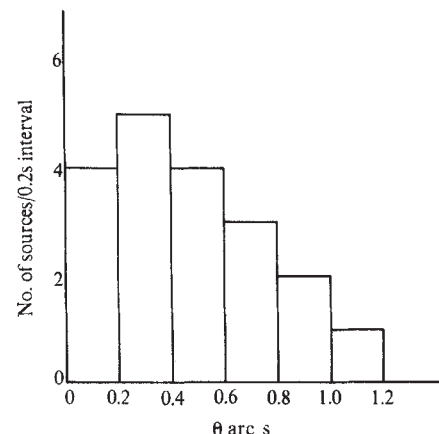


Fig. 3 Measured angular diameters of strongly scintillating sources observed at latitudes $b'' > 30^\circ$. The mean flux density of the scintillating component exceeded the detection limit by at least a factor of five in every case.

Recent pulsar scintillation data show¹⁸ that the angular scattering θ_p along a line of sight to a pulsar at distance L is given by

$$\theta_p^2 = \frac{8c \Delta t}{L} = \frac{4c}{L \Delta f}$$

where Δf is the decorrelation frequency and Δt the pulse broadening. Observations show that both Δf and Δt^{-1} scale approximately as DM^{-2} (where $DM = \int_0^L N_e dl$ is the dispersion measure) according to simple theory. So we obtain

$$\theta_p^2 \propto (DM) \langle N_e \rangle$$

A difficulty arises in applying this method because the pulsar data are somewhat uncertain. As mentioned by Sutton¹⁸, the observed values of Δf and Δt do not satisfy the consistency relation $2\pi\Delta f \Delta t = 1$. For the present we adopt the line shown in Sutton's Fig. 1 as the most probable value and put $\Delta f = 10^7 (DM)^{-2}$ at 318 MHz.

This yields

$$\theta_p^2 = 1.2 \times 10^{-2} (DM) \langle N_e \rangle \text{ at } 81.5 \text{ MHz}$$

where θ_p is measured in arc s. Identifying θ_p with the observed value of θ_s (and doubling because we measure diameters) gives

$$\int_{\text{Sun}}^{\infty} N_e dl = 1.3 [\langle N_e \rangle \sin b^{\text{II}}]^{-1} \text{ cm}^{-3} \text{ pc}$$

If we assume an exponential density distribution with $\langle N_e \rangle = 0.1 \text{ cm}^{-3}$ in the galactic plane, this corresponds to a dispersion measure of $13 \text{ cm}^{-3} \text{ pc}$, for $b^{\text{II}} = 90^\circ$, and a scale height of 130 pc . Identifying θ_s with θ_p may lead to a slight overestimate of the dispersion measure because our observations are not affected by Fresnel filtering near the source.

Galactic Structure

From these preliminary studies it seems that more detailed observations of radio source diameters should throw new light on the distribution of ionized gas in the Galaxy. Measurements at higher frequencies, for example, will require more extended lines of sight in the galactic plane to produce significant scattering, and will therefore probe to greater distances from the Sun, while observations at lower frequencies may give better estimates of the thickness of the plane.

Our observations also have important implications for radio source structure measurements using the very long baseline interferometry (VLBI) method. If lines of sight through the Galaxy are such that the scattering is always "strong", then the limiting angular size caused by scattering alone is as shown in Fig. 5.

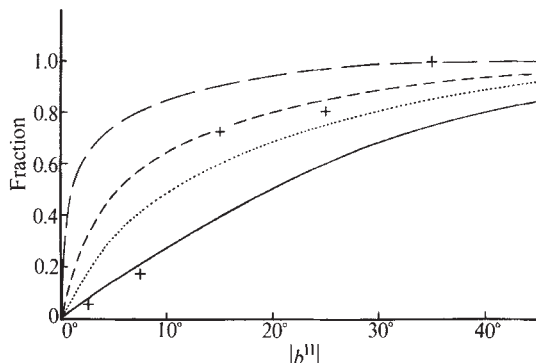


Fig. 4 The observed deficit of scintillating sources at low galactic latitude compared with model calculations. The crosses are observations derived from the left half of Fig. 2 using 5° intervals for $b^{\text{II}} < 10^\circ$. —, $\theta_0 = 0.1$ arc s; ---, $\theta_0 = 0.2$ arc s; ..., $\theta_0 = 0.3$ arc s; — · —, $\theta_0 = 0.5$ arc s.

In Fig. 5 we have also plotted some angular diameter measurements that have been made at different frequencies. We have included only the smallest structure observed at each frequency. The points are taken from observations of interplanetary scintillations made by ourselves and others^{17,19} and from VLBI observations²⁰⁻²⁶.

The data suggest that, for an interferometer of baseline D km, the critical frequency below which the resolution of the interferometer exceeds the scattering of the interstellar medium is given by

$$\nu_{\text{crit}} = \frac{(0.05 \pm 0.02) D}{(\sin b^{\text{II}})^{1/2}}$$

where ν_{crit} is in MHz.

Some existing measurements are already close to this limit, and it appears that intercontinental VLBI at frequencies less than 400 MHz may be unprofitable.

We thank Dr G. G. Pooley for computing the galactic

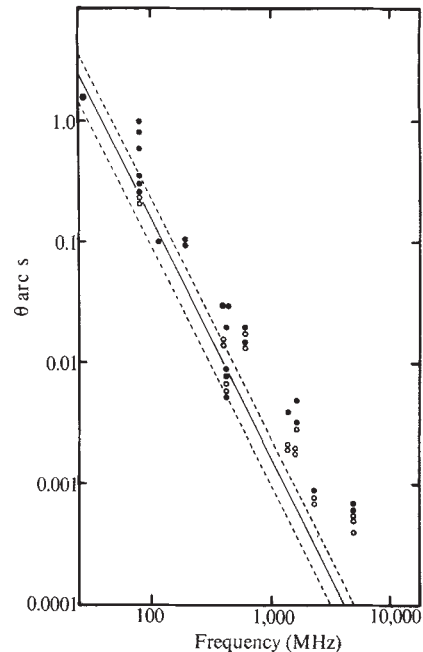


Fig. 5 Angular diameter measurements at different frequencies compared with (frequency-scaled) interstellar scattering derived from θ_0 (81.5 MHz) = 0.25 ± 0.1 arc s. Dashed lines indicate the upper and lower limits. ●, Resolved sources; ○, unresolved sources.

coordinates of the 4C sources. One of us (A. C. S. R.) acknowledges the support of an Isaac Newton Studentship.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Upper Atmosphere Emission Spectrum at Millimetre Wavelengths

THE far-infrared properties of the atmosphere have been studied chiefly in solar absorption measurements from high mountain sites¹⁻³ and aircraft⁴. Gebbie *et al.*⁵ have surveyed the atmospheric emission spectrum from 15 to 60 cm^{-1} from an aircraft. We report here the first observation from aircraft altitudes of the sky emission spectrum between 5 and 20 cm^{-1} , with an instrumental resolution of $\sim 0.1 \text{ cm}^{-1}$.

Our experiment was carried out aboard the NASA Convair 990 aircraft during the Mars opposition flight of August 8 and 9, 1971, near the Hawaiian Islands. Observations were made from an altitude of $\sim 37,000$ feet at approximately local midnight. The apparatus is illustrated in Fig. 1. Sky radiation from the zenith passed through a 2.5 cm thick polyethylene cabin port and was reflected by a mirror through a chopper system, an NPL modular interferometer, and absorbed in a germanium infrared detector operating near 0.3 K. The system was enclosed in a plastic shroud continuously purged by a flow of dry outside air. The sky beam was modulated by the rubber-faced chopping disk shown in Fig. 1, so that the measured signal represents the difference between the ambient temperature black-body radiation of the chopper reference and the sky emission. The optical path is illustrated in Fig. 1b. A 5 cm diameter polyethylene lens L and 0.94 cm light pipe LP provided a $\sim 10^\circ$ field of view and a peak detector signal of $\sim 175 \text{ } \mu\text{V}$. The detector signal and timing pulses indicating advance of the interferometer mirror were recorded on two channels of a Hewlett Packard model 3960 magnetic tape recorder. The resulting aperiodic interferograms were subsequently digitized and Fourier analysed to yield the spectra discussed here.

We analysed an average of the three best interferograms, each comprising a scan of about 14 min, to yield the spectrum shown in Fig. 2. This is compared with the spectrum from a mercury arc source in the laboratory. The noise level is estimated from the zero level sky spectrum between 22 and

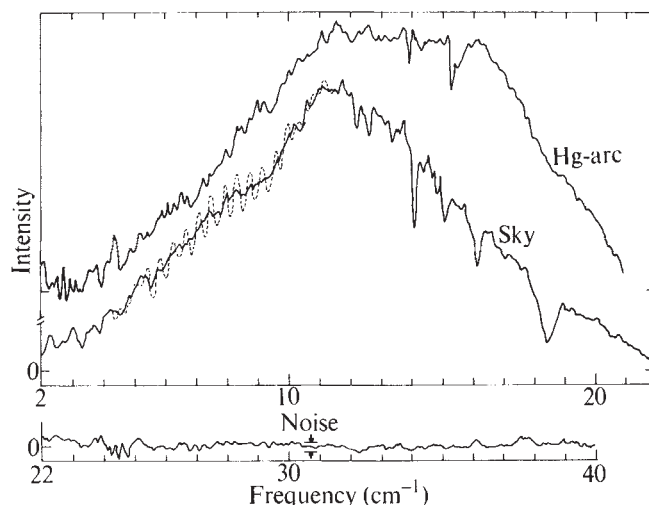
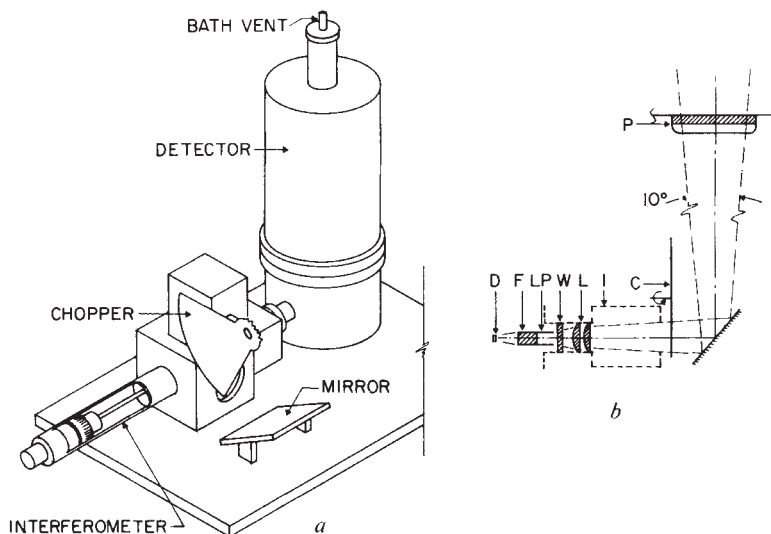


Fig. 2 Triangularly apodized sky and laboratory Hg-arc spectra. The sky spectrum is the average of three scans where the cryostat window interference fringes (dashed lines) have been edited from both curves as discussed in the text. The optical path difference was scanned over 9.6 cm in 801 increments at 1 s intervals for a limiting apodized resolution of $\sim 0.1 \text{ cm}^{-1}$. A 125 μm thick mylar beam splitter was used. Emission features project downward reflecting the decreased sky-reference difference.

40 cm^{-1} , and corresponds to a minimum of about 3% relative to the peak amplitude near 11 cm^{-1} . Interference fringes from the (insufficiently) wedged cryostat window (shown dashed on the sky curve in Fig. 2) have been removed from both spectra by smoothing the corresponding interferogram fringe spike. The mercury arc spectrum in Fig. 2 shows instrumental features at 13.9 cm^{-1} and $\sim 15.2 \text{ cm}^{-1}$. The second of these is large enough to complicate the interpretation of the sky spectrum, and in the normalized spectrum of Fig. 3 this uncertain region is omitted. The spectrum in Fig. 2 shows the characteristic black-body fall-off at low wave numbers as modified by the transmission efficiency of the system. The

Fig. 1 Apparatus. *a*, General arrangement of the system. *b*, Schematic of the optical path where P is the cabin port, C the chopping disk, I the interferometer, L the polyethylene lens, W the cryostat fused quartz window, LP the light pipe, F a 2 cm fused quartz cold filter, and D the germanium detector.



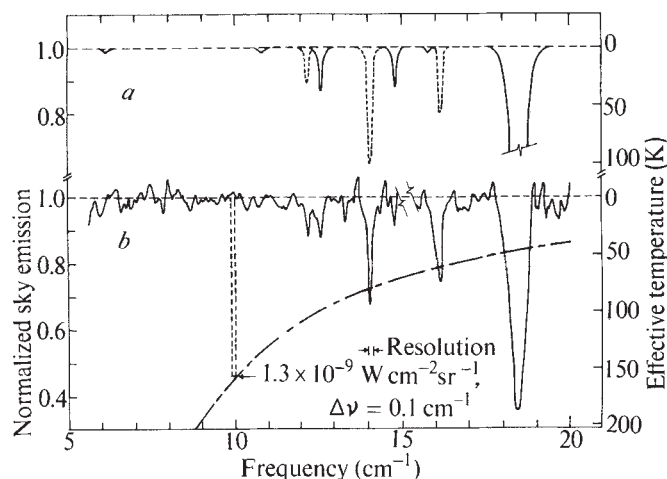


Fig. 3 Experimental and calculated emission spectra. The sky spectrum is shown scaled to a constant amplitude for comparison to the theoretical spectrum. The relative noise level increases away from the measured spectrum maximum of Fig. 2. The effective temperature assumes a chopper reference temperature of 290 K and the calculated curve an air temperature of 220 K, 0.043 atm O₂, and 7.5 μ m precipitable H₂O. The dashed feature at 10 cm⁻¹ indicates the intensity of a resolution limited feature corresponding to 1.3×10^{-9} W cm⁻² sr⁻¹. The frequency variation of this feature is given by the intermittent line. a, Calculated, O₂ ---, H₂O —; b, experimental.

beam splitter and filters F and W combine to produce the decrease above 11 cm⁻¹ and cut-off at ~ 22 cm⁻¹. We normalized this spectrum to constant amplitude (Fig. 3) by estimating the mean background in each interval of wave number. Because the mercury source did not produce a suitable absolute normalization, only narrow line features can be considered significant. The most prominent emission features in this spectral range are the rotational transitions of ¹⁶O₂ and H₂O, as shown by the calculated curve of Fig. 3. For this calculation we convolved the theoretical ¹⁶O₂ Lorentzian line parameters and H₂O Van Vleck-Weisskopf line parameters⁷ with the theoretical instrument scanning function. The precipitable water content was adjusted to fit the observed line strengths and the resultant value of 7.5 μ m can be compared with the average value of 11.7 μ m measured during these scans by the water vapour monitoring device aboard the aircraft.

There is evidence of an additional emission feature at 13.4 cm⁻¹ which has been noted previously in solar absorption by Nolt *et al.*². Our calculations of the expected emission by O₃, ¹⁸O¹⁶O, and N₂O, with O₃ the principal contribution, do not account for the observed magnitude. Neither are other rotational lines of O₃ and N₂O apparent. The identity of the 13.4 cm⁻¹ feature is therefore unclear.

Beery *et al.*⁸ reported evidence of an emission line at 11.7 cm⁻¹ in lower altitude studies, together with an indication of an air mass dependence. This feature is not seen in the spectrum of Fig. 3. Mather *et al.*⁹ saw no evidence of either the 11.7 or 13.4 cm⁻¹ features in their study from White Mountain. The origin of this discrepancy is not clear at present, but further aircraft-borne experiments and some refinements to reduce our noise level should resolve this question.

Finally, we consider the limit these results place on any excess background radiation in this frequency range. Rocket-borne experiments by the Cornell group have shown repeated evidence of an anomalous flux, determined most recently to be $1.3 \pm_{0.15}^{0.1} \times 10^{-9}$ W cm⁻² sr⁻¹ (ref. 10). More recently, however, a Los Alamos rocket experiment¹¹ indicated no background excess below ~ 12 cm⁻¹. Mather *et al.*⁹ report no evidence for any narrow-line emission below 14 cm⁻¹ exclusive of the water masked 12.68 cm⁻¹ region. In Fig. 3 we show the calculated depth for an emission of 1.3×10^{-9} W cm⁻² sr⁻¹ if it had a resolution limited linewidth of 0.1 cm⁻¹. This curve does not take into account any sensitivity roll-off

of the InSb detector used in the rocket experiments above ~ 12 cm⁻¹, so the indicated line depth is a conservative estimate at the higher frequencies. Our aircraft findings are thus consistent with the rocket and ground-based experiments in showing no single narrow-line emission of sufficient strength to explain the large background flux including the 12.68 cm⁻¹ region and extending to 20 cm⁻¹. Some uncertainty remains in the omitted region near 15.2 cm⁻¹ and in the possibility of masking by the strong 18.6 cm⁻¹ water line. Also our method of normalization would not reveal a feature of width greater than ~ 0.5 cm⁻¹ so that a broad feature remains a possibility above the 12 cm⁻¹ cut-off of the Los Alamos observation.

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Variability of Radio Sources and Spectral Type

VARIABLE radio sources are found among quasi-stellar sources, Seyfert galaxies, optical variable sources, sources with one or more compact components and sources with spectra which had an excess of radiation at centimetre wavelengths. The spectrum of a radio source is perhaps more readily determined than the other properties and is a characteristic of primary importance. In particular, observers have always assumed that variability is likely to occur in sources with centimetre excess spectra. We can now establish this relationship without doubt and in a quantitative form: between 90% and 100% of sources with centimetre excess spectra are variable. Furthermore, no source with a normal spectrum shows any evidence of variations. These conclusions are based on observations made during 5.5 yr at the Algonquin Radio Observatory of eighty-four suspected variable sources, each of which has one or more of the characteristics usually associated with variability. Only the association with spectral type has been found to be a dependable indicator of variability. The other characteristics do not show an exclusive relationship with either variability or non-variability.

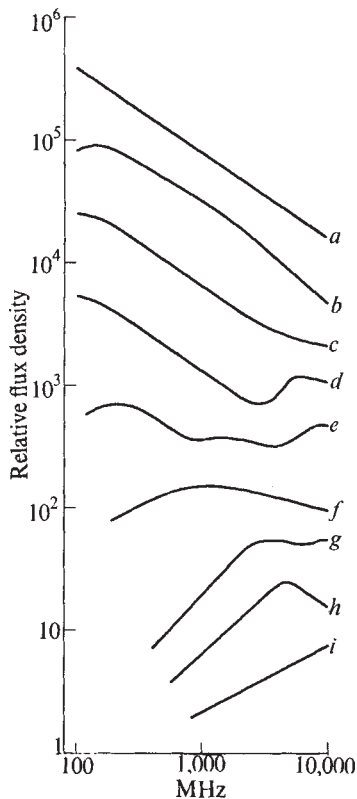


Fig. 1 Representative examples of types of spectra. *a* and *b*, Normal spectra; *c* to *i*, centimetre excess spectra.

By a centimetre excess spectrum we mean a spectrum which has a maximum at centimetre wavelengths or which is typified in the centimetre region by a spectral index $\alpha \lesssim 0.3$ (where $S \propto \nu^{-\alpha}$). There may or may not be a low frequency component of the spectrum in addition to this centimetre component. Fig. 1 shows a number of examples which illustrate the range of spectral types included in this category. It includes all those types of spectra variously described in the literature as enhanced, concave, flat, inverted, unusual, complex or peaked.

Examples of normal spectra are also shown in Fig. 1. Normal spectra we define as spectra of the type widely accepted as typical of non-thermal radio sources, that is, they are of the form $S \propto \nu^{-\alpha}$ within most of the observed range of frequencies ($100 \text{ MHz} \leq \nu \leq 10 \text{ GHz}$): usually the spectral index has a constant value $0.5 \lesssim \alpha \lesssim 0.9$, though there may be a maximum at frequencies below 500 MHz, and/or a slight downward curvature ($\Delta\alpha \lesssim +0.3$) as frequency increases.

Our results, which will be reported in detail elsewhere, are summarized in Table 1. Of the eighty-four sources in the programme, we have omitted from the table eleven for which we have too few observations to establish any trend or type of behaviour. A description of the observing technique and of the equipment will be given in our detailed report. In general, observations are made at intervals of 1 or 2 months and at two frequencies, 6.7 GHz and 10.6 GHz (4.5 cm and 2.8 cm wavelength).

Table 1 shows that of sixty-five sources in the centimetre excess category, fifty-eight are variable, two are probably variable, and only five (PKS 0237-23, DW 0742+10, 3C 216, DW 1548+05, and OW 637) have so far shown no evidence of variability. For a slowly varying source (for example, PKS 2134+004, OQ 208) it can take up to 4 or 5 yr of observation to establish the existence of variability. Of the five non-varying sources two have been observed for only 2 yr, and only 3C 216 has been observed for longer than 3 yr. 3C 216 is a weak source, and variations of small amplitude could have remained undetected. Thus, at most five, and possibly less than five, of the sixty-five sources in the centimetre excess category

may prove to be exceptions to the rule that such sources are variable.

There are eight sources in Table 1 which have normal spectra. All of them have properties which are also found in the variable sources in Table 1: all eight have compact com-

Table 1 Comparison of Centimetre Excess and Evidence of Variability for 73 Sources

Source	Type of spectrum	Evidence of variability ?		No. of years observed
PKS 0106+01	CE	Yes		5
DA 55	CE	Yes		3.5
3C 48	N		No	5.5
CTD 20	CE	Probably		1
PKS 0237-23	CE		No	2
3C 84	CE	Yes		5.5
NRAO 140	CE	Yes		5.5
CTA 26	CE	Yes		5.5
NRAO 150	CE	Yes		4
PKS 0405-12	CE	Yes		4
PKS 0420-01	CE	Yes		4
3C 120	CE	Yes		5
NRAO 190	CE	Yes		5
OF-292	CE	Yes		2
PKS 0458-02	CE	Yes		4
3C 138	N		No	5
OG 050	CE	Yes		2.5
3C 147	N		No	5
OA 198	CE	Yes		3
PKS 0605-08	CE	Yes		4
PKS 0607-15	CE	Yes		4
3C 161	N		No	5
DW 0723-00	CE	Yes		3
PKS 0735+17	CE	Yes		5
PKS 0736+01	CE	Yes		5
OI 363	CE	Yes		4.5
DW 0742+10	CE		No	3
4C-00.28	CE	Yes		2
3C 196	N		No	5.5
PKS 0834-20	CE	Yes		5
OJ 287	CE	Yes		2
PKS 0859-14	CE	Yes		4
3C 216	CE		No	5.5
4C 39.25	CE	Yes		5
OK 290	CE	Yes		4
PKS 1055+01	CE	Yes		5
PKS 1127-14	CE	Yes		4.5
3C 273	CE	Yes		5.5
3C 279	CE	Yes		5.5
3C 287	N		No	5.5
3C 286	N		No	5.5
DW 1335-12	CE	Yes		2
4C 12.50	CE		Probably	2.5
OP-192	CE	Yes		2
PKS 1354+19	CE	Yes		4.5
OQ 208	CE	Yes		4
3C 309.1	N		No	5
OR 103	CE	Yes		2
PKS 1510-08	CE	Yes		4.5
DW 1548+05	CE		No	2.5
DA 393	CE	Yes		3.5
DA 406	CE	Yes		2.5
NRAO 512	CE	Yes		4
3C 345	CE	Yes		5.5
NRAO 530	CE	Yes		5.5
OT 081	CE	Yes		2
3C 371	CE	Yes		4
3C 380	CE	Yes		5.5
OV-213	CE	Yes		2
OV-236	CE	Yes		2
OV-198	CE	Yes		2
OW 637	CE		No	2
3C 418	CE	Yes		5.5
PKS 2134+004	CE	Yes		4
PKS 2145+06	CE	Yes		5.5
OX-192	CE	Yes		2
VRO 42.22.01	CE	Yes		4
4C 31.63	CE	Yes		2.5
PKS 2216-03	CE	Yes		4
3C 446	CE	Yes		5.5
CTA 102	CE	Yes		5.5
3C 454.3	CE	Yes		5.5
PKS 2345-16	CE	Yes		4

CE, Centimetre excess; N, normal.

ponents¹⁻³, seven are quasi-stellar (the exception being 3C 161, a galaxy), and several are optical variables. Yet none of these sources has shown any evidence of radio variability. It therefore seems that none of the above properties is a reliable indicator of potential variability; the only reliable indicator is the spectrum. All known extragalactic radio variables have centimetre excess spectra and all, or almost all, sources with such spectra have proved to be radio variables.

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Search for X-rays above 100 keV from the Vicinity of NP 0527

A PULSATING X-ray source near the Crab Nebula and NP 0527 has been detected recently with a period of 3.8266 s¹. This period is $\sim 2\%$ greater than the period for NP 0527, the radio pulsar which is roughly 1° from the Crab. The observations

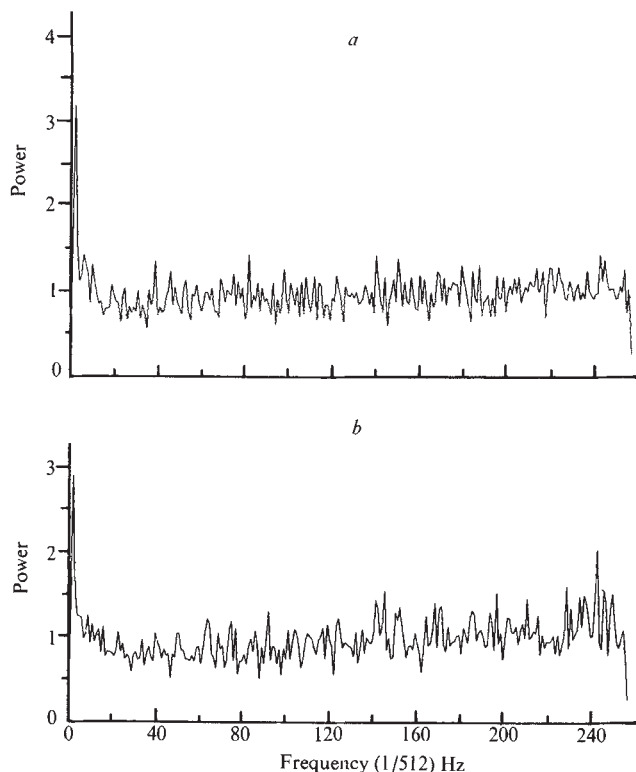


Fig. 1. Summed power spectra for the flight of October 10 (a) and the flight of October 21 (b). No excess power is seen in channel 134 which corresponds to a period of 3.82 s.

were made by a balloon-borne proportional counter in the 30 to 100 keV region.

We describe here a search for evidence of pulsed emission in 100 to 400 keV data obtained on two balloon flights using a large NaI (T1) detector. These data revealed radiation from the Crab Nebula pulsar, NP 0532, with approximately twice the statistical significance obtained by the 30 to 100 keV experiment^{2,3}. If the long period pulsar has a spectrum similar to NP 0532 we would expect it to be observable above 100 keV. The results reported here, from flights on October 10, 1970, and October 21, 1970, which bracket the October 17, 1970, flight of the 30 to 100 keV experiment, show no evidence of pulsation at periods near 3.8266 s.

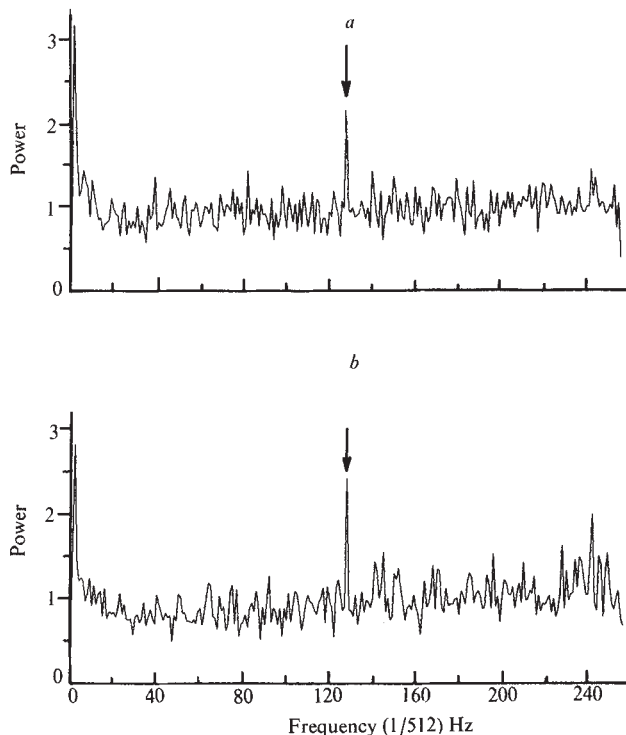


Fig. 2. Summed power spectra for each flight with the addition of a 0.25 Hz component of 1.5 counts s⁻¹.

19,968 s of data from the first flight were divided into 39 segments and 14,436 s of data from the second flight were divided into 28 segments, each segment containing 512 sums each covering 1 s. The Cooley-Tukey Fast Fourier Transform Algorithm was applied to each segment of the data to obtain the power spectrum of the Fourier amplitudes. (The Fourier amplitudes are the absolute magnitudes squared of the complex Fourier coefficients⁴.) Fig. 1a shows the results of summing the 39 power spectra for the first flight and Fig. 1b shows the sum of the 28 power spectra for the second flight. There is no power excess at or near channel 134 which corresponds to a period of 3.82 s. No individual 512 s segment indicates a statistically significant power at this period either.

To estimate the minimum pulsar flux which could have been detected, a simulated flux of 1.5 c.p.s. at a period of 4 s was added to the data by Monte Carlo methods. This corresponds to a flux at the top of the atmosphere of 4×10^{-3} photons cm⁻² s⁻¹. These results are shown in Fig. 2a and 2b for the October 10 and October 21 flights respectively. The excess power at 0.25 Hz (channel 128) is clearly evident.

We conclude that no emission from the new pulsar is observed above the minimum level of sensitivity. The combined observations permit only one of the following interpretations of the 30-100 keV data. First, the new pulsar may have

a significantly different spectrum from that of NP 0532, but the present observation coupled with the MIT rocket results⁵ rule out any continuous emission with a power-law spectrum. Second, the pulsar may be variable on longer time scales, but if this were true for a Crab-like spectrum definite limitations on long-term variability would be imposed by the lack of such variability within eight hours (the length of the present observations) and the failure to detect the pulsar seven days before and four days after the 30–100 keV observation. Third, it is also possible that the apparent 30–100 keV pulsation is not real, and confirmation of that result is needed, as well as spectral data in the 30–100 keV region.

Because the detector used for the present result was omnidirectional, the results also place an upper limit on any other periodic X-ray source in the region of the sky viewed. This upper limit corresponds to a flux at the top of the atmosphere of approximately 3×10^{-3} photons $\text{cm}^{-2} \text{ s}^{-1}$ in the 100–400 keV region, and applies to periods from 2 to 100 s for sources with right ascension between 2 and 14 h and declination between 0° and $+60^\circ$.

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Optical Counterpart of Cen X-3

WE wish to suggest the possibility that the X-ray source Cen X-3 may be identified with the Algol-type variable LR Cen, based on the proximity of their coordinates^{1,2} and, more significantly, on the close coincidence of the orbital periods of the objects (2.08712 days for Cen X-3 (ref. 1) and 2.095595 days for LR Cen (ref. 2)). The 0.4% difference between the periods may well result from a change in period between 1930 and 1971 or from errors in the X-ray observations. The coincidence of the elements of LR Cen and Cen X-3 could be a proof of identity but unfortunately there have been no data obtained for LR Cen since 1930. Photometric and spectroscopic observations are urgently needed. Accepting that the observations are of the same system, we determine some of the characteristics of the system.

We used the mean photographic light curve based on observations made between 1922 and 1930 (ref. 2) to provide the photometric orbital elements. The light curve is symmetric, of the Algol type; the light intensity between eclipses is given by the expression: $I = 1 - 0.03 \cos \theta - 0.02 \cos^2 \theta$. The second-

dary minimum occurs midway between primary minima, and the eclipses are partial. The normalized depths of the minima are 0.670 and 0.107.

The orbital elements were obtainable only for the occultation at the primary minimum. Using Russel's method (assuming circular orbit, thin atmospheres and uniform disks), we determined them as:

$$k = 0.62, \alpha_{\max}^0 = 0.965, i = 81^\circ 05', \tau_1 = 0.352, \tau_2 = 0.218, \\ L_1 = 0.305, L_2 = 0.695.$$

In deriving these elements we used only the slope and depth of the primary minimum and the depth of the secondary. Variables have their usual meaning (see, for example, ref. 2).

The width of the secondary minimum seems to be ~ 1.5 times larger than that of the primary minimum.

These results and the Uhuru data communicated to us by E. Schreier and colleagues lead to the following conclusions: Cen X-3 is a close binary system, its brighter and smaller component being connected with the X-ray source the minima of the intensity of the X-ray source are connected with absorption or scattering in the atmosphere of the more massive and relatively dark component; most probably, the mass of the dark component is 16 to 20 $M_\odot$ and its radius is $\sim 7 R_\odot$, that is, this is a main sequence O8–B0 star with surface temperature of $\sim 25,000$ K; and the surface temperature of the brighter component must be $\sim 250,000$ K, with the Rayleigh-Jeans region of the spectra of each component being observed.

It seems improbable that the whole spectrum is Planckian, because the total power of such radiation would be $L_2 = 10^{42}$ erg s^{-1} ($L_2 = 4\pi r_2^2 \sigma T_2^4$, where $r_2 \approx 2.4 \times 10^{11}$ cm and $T_2 \approx 250,000$ K). This is evidently unacceptable.

So somewhere in the near ultraviolet the bright component becomes optically thin. It may be that a hot plasma shell surrounds the compact X-ray source. The optical depth for free-free transition at $\nu = 10^{15} \text{ s}^{-1}$ is ≈ 1 , the mean N_e for the brighter component is $\sim 10^{-12} \text{ cm}^{-3}$ and the mass of the shell $\sim 10^{23}$ g. These are rough estimates because physical conditions in the plasma shell depend on the distance from the X-ray source in its centre. Apparently the non-pulsating X-ray component originates in this shell from Compton scattering of the pulsating component of the radiation.

The brighter component of the radiation from Cen X-3 comes from a plasma, the temperature of which is determined by the X-ray absorption. We think it unlikely that this radiation is related to accretion of gas ejected from the B star onto the compact object. The B star is well inside its Roche limit and is nearly spherical. Most probably outflow of gas takes place from the compact X-ray source; our model of this source is therefore similar to the "double-component" model of Sco X-1 (ref. 3).

This young star may well lie in a spiral arm, and Cen X-3 may be in the Car-Sgr-Sct arm at a distance of 2 to 2.5 kpc, if $A_V \sim 3$ mag. Nearby clusters in the same arm have A_V up to 2 mag (ref. 4). Perhaps the object is more distant, but in this case its X-ray luminosity must be essentially higher than that of NP 0532 (CM Tau).

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Identification of Cen X-3

X-RAY observations of the pulsating X-ray source Cen X-3 indicate¹ that it is a component of an eclipsing binary with a circular orbit of radius 0.08 AU. The position and period for Cen X-3 and for its suggested optical counterpart, LR Cen, are given in Table 1. There is a slight discrepancy in the agreement of the positions and periods. Observations are under way to redetermine the optical period.

Table 1 Position and Period of Cen X-3 and LR Cen

	α (1950)	δ (1950)	Period
Cen X-3 (from ref. 2)	11 h 19.0 $\pm$ 0.4 min	-60° 19.2' $\pm$ 3'	2.087 days
LR Cen (from ref. 2)	11 h 21.6 min	-60° 33.7'	2.096 days

Photometry of the variable star LR Cen with the 40-inch reflector at the Siding Spring Observatory is shown in Fig. 1. The light curve of LR Cen is typical of an eclipsing binary of the Algol type; the equal spacing of the primary and secondary minima indicates that the orbit is circular and the lack of any constant phase indicates that the eclipse is partial. The time of mid-eclipse of the X-ray source, as calculated from the X-ray data, is also shown in Fig. 1. It seems that the eclipse of the X-ray source corresponds to the eclipse of the fainter star. Note that a period of 2.090 days, a value midway between the two values given in Table 1, would make the predicted time of the X-ray source eclipse coincide with time of the secondary minimum of LR Cen. Consideration of the colour changes during primary eclipse, along with the shape of the light curve, leads me to the conclusion that the X-ray source is the redder star of the pair, and that it has a low surface brightness with considerable limb darkening. The brighter star is blue with more nearly constant surface brightness.

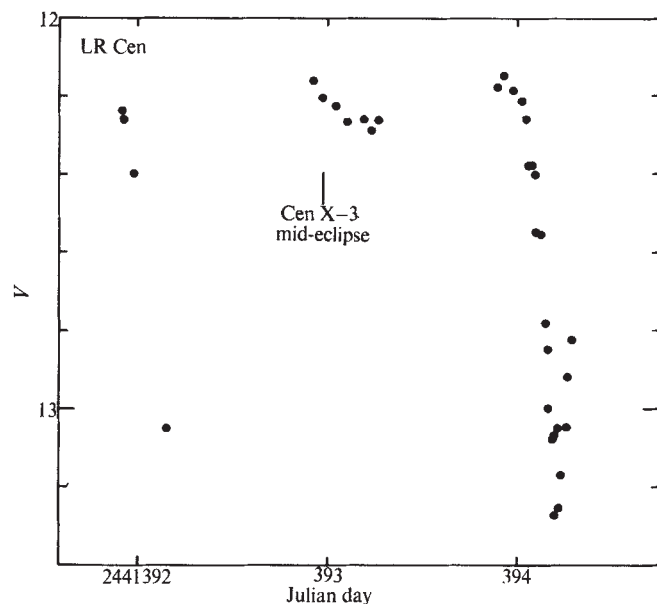


Fig. 1 Light curve of LR Cen.

A search for light variations with a period of 4.84 s, the period of the X-ray pulses, was made during the primary eclipse when the X-ray source was in front of the brighter star. The results of successive 2 s integrations are shown in Fig. 2. The error bars shown include systematic effects that influence the determination of the zero point as well as random errors from photon statistics which form only 1/3 of the total error shown. Light variations with the period of the X-ray pulses may be present with an amplitude of 2%, but because

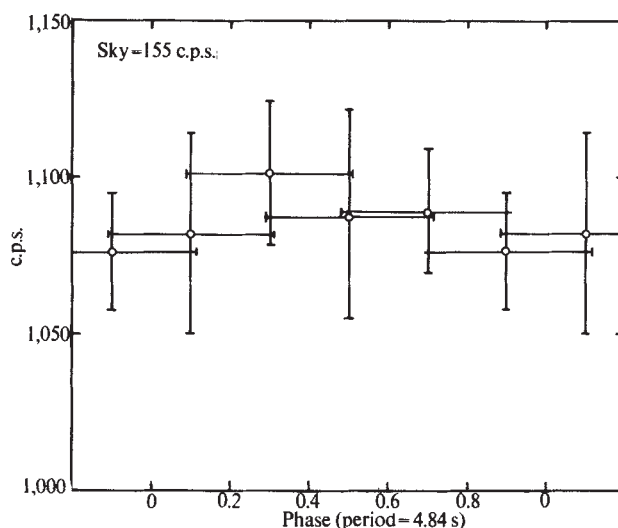


Fig. 2 Light variation of LR Cen with the frequency of the X-ray pulses. Random errors are 1/3 of the total errors shown.

of the large errors in the data, this will require further confirmation before it can be regarded as established. The data set an upper limit of 10% to the amplitude of light variation with a 4.84 s period.

My attention was drawn to LR Cen as a candidate for optical counterpart of Cen X-3 by Dr J. Kristian.

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Published Analyses of Igneous Rocks

THE most comprehensive published collection of analyses of igneous rocks¹ contains more than 7,600 entries considered "superior" by the author. One of us (R. W. Le M.) has a comparable file of 16,050 published analyses, and F. C. has a file of 10,494 published analyses of Cainozoic volcanic rocks alone. Comparison of our files, referred to below as the C and L files, provides some basis for estimating how much information of this type has now been published.

The C file is restricted to analyses of Cainozoic volcanic and related hypabyssal rocks and includes only what petrologists usually consider "complete" analyses, that is those in which the analyst reports values for SiO₂, Al₂O₃, Fe₂O₃, FeO, MgO, CaO, Na₂O, K₂O, MnO, TiO₂, P₂O₅ and H₂O—recorded in the conventional two-decimal fashion. The L file contains analyses of igneous rocks of all kinds and ages, and although most of the analyses in it satisfy criteria set up for the C file, neither "completeness" nor citation to hundredths of a per cent is a condition for inclusion. If each file were based on an exhaustive literature search, file C would be a subset of file L. If the actual literature searches were independent, the predicted number of published analyses of volcanic and associated hypabyssal rocks to be found in an exhaustive literature search would be $N_v = N_C N_L / N_B$, where N_C is the number of analyses in file C, N_L the number of analyses of volcanic and associated hypabyssal rocks in file L, and N_B the number of analyses common to both files.

For some of the more common volcanic rocks N_C , N_L and N_B have the values shown in Table 1. Since there is no

Table 1 Frequencies of Occurrence of Some Common Rock Names in Files C and L

Rock	N_C	N_L	N_B
Basalt	2,328	1,970	617
Andesite	2,197	1,746	642
Trachyte	359	388	138
Rhyolite	430	469	67
Phonolite	252	233	85
Sum	5,566	4,806	1,549
All volcanics and hypabyssals	10,494	7,918	

a priori reason to suppose that the number of coincidences varies systematically with rock type, the appropriate estimate of $\hat{N}_V$ is $(5,566)(4,806)/1,549 = 17,269$ if we ignore the fact that file L is not confined to Cainozoic materials. The tallies in Table 1 include only analyses of rocks in whose source names the principal noun is as shown; this excludes 47% of the C file and 39% of the volcanic portion of the L file. Extending the estimate of $\hat{N}_V$ to cover volcanic and hypabyssal rocks regardless of source names, $\hat{N}'_V = (7,918/4,806)17,269 = 28,451$ as estimated from the L file or $(10,494/5,566)17,269 = 32,559$ as estimated from the C file. Using the average of these values, 30,505, and assuming, further, that extent of literature coverage does not vary systematically with rock name or rock type in file L, the estimated number of published analyses of igneous rocks of all kinds is $(16,050/7,918)30,505 = 61,834$. We suspect that this is a considerable underestimate, for the reason that although there has been no collaboration in preparation of the files, both of us have relied heavily on certain secondary sources; this would tend to generate a value of N_B greater than would have been found if we had independently searched the primary sources involved. A compilation which did for modern igneous petrology what Washington¹ did for the petrology of the period before the First World War would almost certainly contain more than eight times as many analyses. It could well be that published information of this sort has increased by more than an order of magnitude in the past 59 years.

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BIOLOGICAL SCIENCES

Smoking, Platelets and Thrombosis

SEVERAL reports suggest a relationship between smoking and coronary heart disease¹⁻⁴, and in view of the role played by platelets in thrombosis⁵⁻⁸ it seemed important to examine the effect of cigarette smoking on platelet function. Although there have been a few studies of the platelets of cigarette smokers, most have not been compared with non-smoking data⁹⁻¹⁵.

Healthy male volunteers were divided into three groups of ten. Group 1 consisted of non-smokers, group 2 were light smokers (daily consumption less than twenty cigarettes) and group 3 were heavy smokers (more than twenty a day). Volunteers had not ingested aspirin for at least 10 days¹⁶⁻¹⁸ and had only a light, fat-free breakfast. To minimize variability one

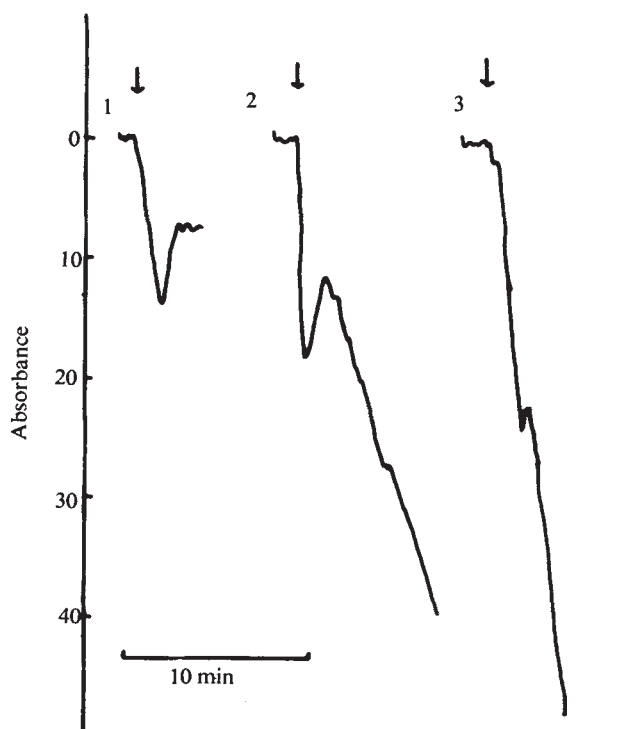


Fig. 1 Change in absorbance induced by ADP ($1.7 \mu\text{g/ml.}^{-1}$) in PRP from smoking and non-smoking subjects. 1, Non-smoking; 2, light smoking; 3, heavy smoking.

subject from each group was examined each morning, being matched as nearly as possible for age and smoking habits.

To look for chronic changes in platelet function in the smoking subject, blood was withdrawn after a 2 h non-smoking period. Acute change resulting from smoking one cigarette of the subject's normal brand was determined by comparison of platelet function immediately and 15 min after a cigarette with that before smoking, and in matched non-smoking controls.

Aggregation of platelets induced by ADP was examined by Born's optical density method¹⁹, and rates of aggregation and de-aggregation were measured from the chart record described by Baumgarten and Born²⁰. Platelet micro-electrophoresis was carried out using a cylindrical chamber (Rank Bros, Cambridge)²¹, with a capacity of 1 ml. and solid silver/silver chloride electrodes. The thromboelastograph (TEG) was designed and described by Hartert²² and the measurements associated with thrombus formation made from the photographic recording were analysed statistically.

Platelets from smoking subjects seemed to be more active when aggregated with ADP. A constantly greater change in absorbance was observed in these samples, indicating more platelet clumping. When samples from each group were compared, a lower concentration of ADP was required in the smoking groups to induce the second phase—permanent platelet aggregates—with the release of platelet constituents important in further aggregation and coagulation *in vivo*. There was an increase in the rate of secondary aggregation but no difference in the rates of primary aggregation or de-aggregation. These differences were apparent when platelets were withdrawn immediately after smoking and compared at the same time with platelets from non-smoking subjects. Platelets tested during the non-smoking period had similarly enhanced activity compared with non-smoking data only in older subjects with the longest history of heavy smoking. A typical series of results is given in Fig. 1. There was no significant difference in the platelet count in any group.

Micro-electrophoresis of the platelets from all groups revealed no significant difference between platelet mobilities of non-smokers and smokers during a non-smoking period. A typical biphasic mobility change was also demonstrated in all

Table 1 Platelet Electrophoretic Mobility

Sample	Electrophoretic mobility $\mu\text{s/V/cm}$				
	Control	ADP $\mu\text{g ml}^{-1}$		Noradrenaline $\mu\text{g ml}^{-1}$	
		0.05	0.5	0.05	0.5
Non-smoking control group	1.07	1.17	1.00	1.14	0.96
Light smoking group					
(1) Pre-smoking	1.06	1.16	0.94	1.15	0.99
(2) Post-smoking					
0 min	1.07	1.07	1.00	1.06	0.99
15 min	1.09	1.18	0.99	1.16	1.02
Heavy smoking group					
(1) Pre-smoking	1.09	1.16	0.99	1.17	1.10
(2) Post-smoking					
0 min	1.06	1.05	0.96	1.06	1.00
15 min	1.04	1.10	0.98	1.12	0.93

groups after incubation with the aggregating agents ADP and noradrenaline²³.

This biphasic pattern was, however, altered after subjects had smoked one cigarette. Incubation with $0.05 \mu\text{g/ml}$ of both ADP and noradrenaline induced no increase in electrophoretic mobility of platelets from all the heavy smoking group, and in eight of the ten light smoking group, when samples were withdrawn immediately after smoking. Incubation with the high concentration of these substances ($0.5 \mu\text{g/ml}$) induced the normal response in all subjects. Platelet response 15 min after smoking returned to normal in all but three of the heavy smokers, and in all the light smokers.

The response of platelets to ADP immediately after smoking is shown in Fig. 2. (The average results of all of group 3 have been plotted and the eight of group 2 showing the same response.)

When compared with non-smoking results, the coagulation time of whole blood of smokers during a non-smoking period, measured by thromboelastography, was significantly shorter. This was significant at the 0.001% level in heavy smokers and at the 5% level in light smokers.

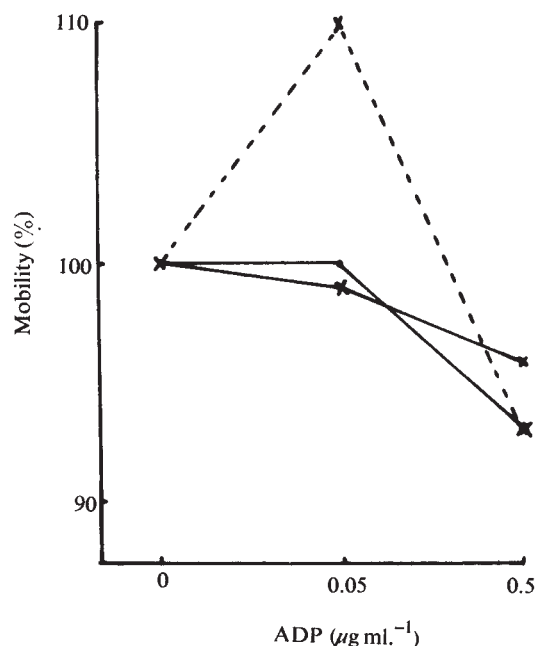


Fig. 2 Electrophoretic mobility response induced by ADP (0.05 and $0.5 \mu\text{g ml}^{-1}$) in non-smoking and smoking subjects immediately after one cigarette. $\times$ --- $\times$, Non-smoking control; $\times$ — $\times$, light smoking; $\bullet$ — $\bullet$, heavy smoking.

Increased platelet function was also reflected in the TEG. The number and function of platelets are important in determining the maximum amplitude of the TEG recording²⁴ and the platelet contractile protein, thrombosthenin, provides the mechanical basis for clot retraction and contraction during "viscous metamorphosis"²⁵. In the heavy smoking group there was an increase in maximum tensile strength of the clot significant at the 5% level when compared with the clot strength of non-smokers, and a decrease in the time taken to read this

Table 2 Average Thromboelastography Measurements. Comparison of Cigarette Smokers with Non-smokers

Sample	<i>r</i>	<i>k</i>	<i>r+k</i>	Σ	<i>t</i> Σ	<i>R1</i>	<i>R2</i>
Non-smoking Control	7.08	5.40	12.48	7.650	34.50	37.00	122.50
Pre-smoking Light group	6.51*	5.37	11.88	79.00	29.80†	54.20*	145.90
Pre-smoking Heavy group	5.79‡	4.92	10.71‡	92.10†	29.40‡	47.90	150.30*
Post-smoking 0 min Light group	6.50*	5.30	11.80	72.80	31.16*	49.80	136.10
Post-smoking 0 min Heavy group	5.67‡	4.60	10.07‡	88.30*	27.30*	65.30‡	161.70†
Post-smoking 15 min Light group	6.31*	5.00	11.31	77.00	31.16*	57.16*	150.0
Post-smoking 15 min Heavy group	5.81‡	4.83	10.64*	84.16	29.00‡	63.33‡	164.50†

r, Thromboplastin generation time (or reaction time); *k*, clotting speed; *r+k*, total coagulation time; Σ , maximum thrombus strength; *t* Σ , time taken for Σ ; *R1*, *R2*, clot retraction.

* 5% significance level.

† 1% significance level.

‡ 0.001% significance level.

Table 3 Thromboelastograph Measurements. Long-term Heavy Cigarette Consumption compared with Non-smokers

Sample	<i>r</i>	<i>k</i>	<i>r+k</i>	Σ	<i>t</i> Σ	<i>R1</i>	<i>R2</i>
Non-smoking Control	K. H.	7.11	5.25	12.36	85	37	125
	L. M.-T.	7.18	5.20	12.18	79	34.5	140
	J. C.	7.9	5.5	13.4	76	33	139
Post-smoking 0 min Heavy group	P. H.	5.29	3.5	8.79	104	22	235
	A. S.	4.31	3.5	7.81	113	28	170
	J. L.	5.7	4.0	9.7	92	75	165

For explanation see legend to Table 2.

value, significant at the 0.001% level. The rate of initial clot formation was also increased, as was clot retraction. The values obtained immediately and 15 min after one cigarette showed no significant differences from the results of the same subject before smoking, although they differed significantly from non-smoking control data.

As with the other parameters, TEG recordings showed most change in the oldest heavy smoking subjects with the longest history of smoking. TEGs of a control non-smoker and a heavy smoker, immediately after one cigarette, are shown in Fig. 3.

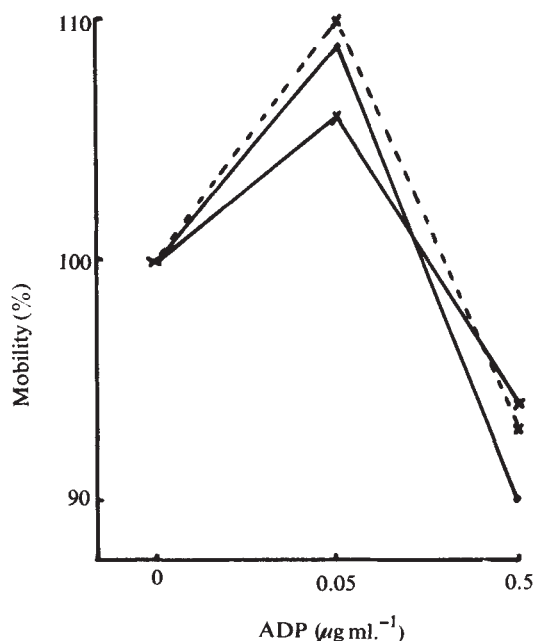


Fig. 3 Thromboelastograph recording of non-smoking subject (x---x) compared with a heavy smoker (●—●). x---x, light smoking.

Variation occurred within groups. The ages of the light smoking group 2 ranged from 18–51 with a “smoking life” of 1–30 yr, and in group 3 heavy smoking subjects were aged between 16 and 56, smoking for 2–35 yr. All results from smokers were more significant when compared with the oldest non-smokers with the longest history of smoking.

Changes in platelet activity in the conditions of this experiment could be separated into those apparent only after smoking and those detectable in the heavy smoker even after a non-smoking period. Thus, the biphasic platelet electrophoretic mobility change was altered only after smoking, and had returned to normal after 15 min. On the other hand, statistically significant changes in TEG measurements were apparent in heavy smokers, even during a non-smoking period. Coagulation, the rate of initial clot formation, the maximum thrombus strength, its rate of formation, and clot retraction, all showed adverse changes with might lead to a hypercoagulable or hyperthrombotic state compared with the non-smoker. No difference in the parameters measured was apparent after smoking when compared with the values obtained before cigarette consumption.

Both acute and chronic changes seemed to result in the same alteration of platelet aggregation in the heaviest smoking subjects. The greatest differences, however, were in the acute, immediate post-smoking values, when increased sensitivity to ADP was suggested by a constantly greater change in absorbance, and a constantly lower concentration of stimulus necessary to induce permanent aggregation compared with a non-smoker.

Relatively few subjects were tested with varied environmental and smoking histories. But it seems from the results obtained in this particular experiment that there is a greater risk of the

formation of haemostatic plugs and the build-up of a thrombus in thrombogenic conditions in the smoking subject. Such a potentiating effect has also been suggested in the speculated role of oral contraceptives in the pathogenesis of thromboembolism²⁶.

Similar results have been obtained in the study of platelet function in animals (R. I. H., unpublished).

It is not clear whether the smoke components—either the particulate “tar” and nicotine phase or the volatile gas-vapour phase—directly affect platelet function, or whether the changes demonstrated reflect changes in blood biochemistry. It has been suggested that electrophoresis reflects changes in the plasma systems which effect platelet behaviour rather than platelet abnormality, and changes in the plasma low density lipoprotein have been shown to alter platelet electrophoretic mobility²⁷. Smoking has been shown to increase plasma non-esterified fatty acids²⁸ and a significant increase in cholesterol β and pre- β -lipoproteins has been reported²⁹. Both fatty acids and β -lipoproteins enhance aggregation of human platelets^{30,31} and cholesterol feeding enhances aggregation in rabbits (R. I. H., unpublished).

It is also possible that platelet activity is altered by the 5-hydroxytryptamine liberated by nicotine^{32–34}, and experiments are in progress to elucidate this problem.

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Effect of Temperature on the Equilibrium Endplate Potential

It has been suggested¹ that during the interaction of acetylcholine (ACh) with the cholinergic receptor (ChR) each elementary ionic channel of the postsynaptic membrane (PSM) becomes active, that is accessible for K⁺ and Na⁺ ions, for such short a time that the equilibrium potential (E_p) of PSM is determined by a current which is far from stationary. This suggestion—the hypothesis² of “transient state of ionic channel”—arose from the finding^{3,4} that E_p of the PSM is described by

$$E_p = \frac{RT}{F} \left\{ \ln \frac{[K]_o}{[K]_i} + \frac{\Delta g_{Na}}{\Delta g_K} \ln \frac{[Na]_o}{[Na]_i} \right\} \left\{ \frac{1}{1 + \frac{\Delta g_{Na}}{\Delta g_K}} \right\} \quad (1)$$

in wide range of external concentrations of K⁺ and Na⁺ ions, where $[K]_o$, $[Na]_o$, and $[K]_i$, $[Na]_i$ are the external and internal K⁺ and Na⁺ concentrations respectively; Δg_K and Δg_{Na} are PSM conductances for K⁺ and Na⁺ ions respectively; R is the gas constant, F is the Faraday number and T is the absolute temperature.

Equation (1) predicts that the ratio $\Delta g/\Delta g_K$ of an activated PSM does not depend on a wide range of charges of concentration of these ions in the external solution. This behaviour is radically different from that of other well studied excitable surface membranes, the E_p of which obeys the Goldman equation⁵:

$$E_p = \frac{RT}{F} \left[\ln \frac{P_K [K]_o + P_{Na} [Na]_o + P_{Cl} [Cl]_i}{P_K [K]_i + P_{Na} [Na]_i + P_{Cl} [Cl]_o} \right] \quad (2)$$

where P_K , P_{Na} , and P_{Cl} are the coefficients of the membrane permeability for potassium, sodium, and chloride ions respectively.

From the hypothesis of “transient state of ionic channel” it has been proposed that some cholinomimetics open ionic channels on the PSM for longer than does ACh. In this case the behaviour of E_p of PSM would follow equation (2) and not equation (1). Such a situation was discussed by Dunin-Barkovski *et al.*², dealing with the action of ACh, carbocholine, dithiline, decamethonium and subcholine (D_6) on E_p of the endplate PSM of frog sartorius muscle. With subcholine (D_6) E_p seemed to follow equation (2). This was interpreted as a result of ionic channels operating during the action of D_6 in a stationary regime.

Here, we examine the hypothesis of “transient state of ionic channel” at low temperature. It is natural to expect a considerable temperature drop to increase the time of the opening of an elementary ionic channel and E_p would be predicted by the Goldman equation.

To determine the relation between E_p and the ionic concentration, E_p values were compared at $[K]_o = 2.5$ mM and $[K]_o = 0.5$ mM (ref. 2). For such a variation in ionic composition of the medium equation (1) predicts the displacement of E_p by 18 mV, while equation (2) only predicts a change by 1 mV.

The experiments were carried out on sartorius nerve-muscle preparations of the frog, *R. temporaria*, treated with curare (5×10^{-6} – 10^{-5} gm l.⁻¹).

Registration of the endplate potential (EPP) and the intracellular polarization of muscle fibres in the endplate region were performed with conventional microelectrodes by routine techniques². E_p was estimated from the series of membrane potential values, and corresponding EPP amplitudes by linear approximation with the least squares method, at a point of the membrane potential corresponding to the zero value of EPP. For the calculations the only fibres chosen were those with resting potentials of no less than 40 mV at the end of the experiment.

Two solutions were used: normal Ringer (116 mM NaCl; 2.5 mM KCl; 5 mM CaCl₂, pH 7.3) and that containing 0.5 mM KCl instead of 2.5 mM KCl. The cooled solution was perfused through the chamber containing the nerve-muscle preparation

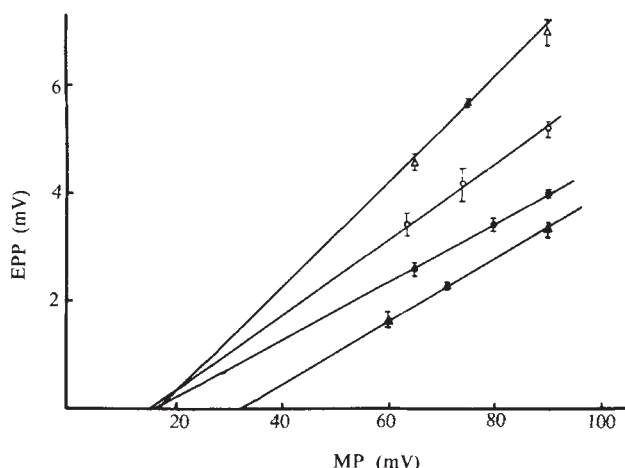


Fig. 1 Relations between EPP (ordinate) and MP (abscissa) at various temperatures. ○△, Temperature (T) = 2° C; ●▲, temperature = 26° C; ○●, the normal Ringer; △▲, $[K]_o$ reduced five-fold.

at a rate of 25 ml. min⁻¹. The temperature was controlled with a thermocouple mounted in the chamber.

Fig. 1 shows the E_p of the same fibre at different temperatures. At 2° C values of E_p at $[K]_o = 2.5$ mM and at $[K]_o = 0.5$ were practically coincident. When the temperature of the external solution was raised to 26° C, E_p in the solution having $[K]_o = 2.5$ mM did not change while at $[K]_o = 0.5$ mM, E_p seemed to be displaced by 15 mV towards the hyperpolarization of the membrane. E_p was also determined at low and high temperatures on different fibres (Table 1).

For each of the fibres at low temperature, values of E_p in solutions of normal concentration of potassium ions do not differ appreciably from those at $[K]_o = 0.5$ mM (the mean values for all the fibres differ by no more than 1 mV). The control experiments carried out at a high temperature show that the decrease of $[K]_o$ from 2.5 mM to 0.5 mM causes a shift in E_p of 18 mV, on average, being close to that obtained by Takeuchi³ in similar conditions (17 mV).

The results indicate, first, that E_p values for an external potassium concentration of 2.5 mM are the same at low and high temperatures and, second, that the behaviour of the equilibrium potential at low temperatures corresponds to equation (2) more than to equation (1).

We suppose that the decrease in temperature leads to the increase in the action time of ionic channels. Based on the hypothesis of “transient state of ionic channel” this would mean that at low temperature the ionic channels of an activated PSM keep open for sufficient time to reach the stationary ionic flow. There are two alternative explanations for a change-over in the channel action from a non-stationary machine to a stationary one: (1) the time the channels keep open corresponds to the lifetime of the ACh–ChR complex which increases with decreasing temperature; (2) the temperature decrease slows the action of the operating mechanism, that is of those conformational changes which arise from the origin of the ACh–ChR complex, and leads to an increase in the membrane permeability. It is important to decide between these two suggestions, for they indicate the action mechanism of a postsynaptic membrane whether the ionic channels open for the whole lifetime of the complex with ACh, or whether the mediator only acts as a trigger.

The effect of a mediator on ChR at low temperature coincides with that of D_6 on ChR at high temperature for two independent features. In both cases the E_p values in a solution of the normal ionic composition coincide with those of the EPP at high temperature. When $[K]_o$ is decreased five-fold the equilibrium potential for D_6 does not change, nor does E_p at low temperature.

Both these effects probably take place in this process, but the

comparison with the results at low temperature and those for D₅ at high temperature points to the validity of the first assumption: that is the time the channels keep open corresponds or coincides with the lifetime of the complex of ACh or its analogues with ChR.

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Changes in Organochlorine Contamination of the Marine Environment of Eastern Britain monitored by Shag Eggs

A SURVEY of the residues of organochlorine insecticides in the marine environment of north-east England and south-east Scotland in 1965 and 1966 showed that dieldrin and *pp'*-DDE (a metabolite of DDT) occurred in the highest amounts¹. The highest concentrations of residues were found in tissues and eggs of the shag and cormorant (*Phalacrocorax aristotelis* and *P. carbo*), with, for example, the eggs of the former possessing on average almost 1 p.p.m. of dieldrin and about 2 p.p.m. of *pp'*-DDE. All other organochlorine insecticides including DDT were present at concentrations of less than 0.02 p.p.m. As a result of this survey it was concluded that shags' eggs were the most suitable material for continuing the monitoring of residues in that environment. The criteria used in selecting the indicator species were, first, that the concentration in the indicator should be sufficiently large to allow changes to be readily detected; second, that the movement of the organism from which the sample was to be taken should be restricted to the area under study; third, that the diet (and source of contamination) should be known; fourth, that the relationship

between variations in age of the organism and the concentration of insecticide should have been determined.

The shag in north-east England and south-east Scotland meets many of these requirements. It has a high body concentration of these residues and the concentrations of organochlorine compounds in birds' eggs are known to be related to those in body tissues². Extensive marking studies on shags have shown that the adults typically range only 20–40 miles from their breeding area and studies in progress continue to monitor any change in the distribution of the adults³. After the first few months of life, no further age-related accumulation of insecticides in the tissues of shags was found⁴.

Samples of shags' eggs have been collected (under licence) from the Isle of May, Fife, Scotland, and the Farne Islands, Northumberland, between 1964 and 1971. The mass mortality of adult shags on the Farne Islands in 1968⁴ and their subsequent slow recovery resulted in only small samples being available from the Farne Islands from 1968. Fortunately, enough information has been collected to show that the concentrations of organochlorine insecticides in the shags' eggs from the Isle of May are similar to those in eggs from the Farne Islands, and in the absence of consistent and significant differences in the means and trends between the two areas the data from both have been pooled.

The method of determination of organochlorine insecticides in the egg has been described previously^{1,5}. There is a loss of water (but not organochlorine insecticides) from eggs during incubation, and so all the concentrations were corrected to those in the newly laid egg by determining the egg volume within the shell⁶. As the distribution of concentrations in each set of samples was positively skewed, the results for the samples for various years have been summarized as geometric means.

In some years there are marked variations in the concentrations of organochlorine insecticides in eggs laid at different dates during the breeding season¹. In all but one year the eggs were collected throughout the breeding season and the residues from year to year are therefore comparable, but in 1964 the majority of the eggs were collected in the latter part of the breeding season. It is therefore probable that the recorded concentrations in that year may not be fully representative of the breeding population.

The results are presented in Table 1 for samples collected during the years 1964 to 1971. Only dieldrin and *pp'*-DDE are recorded because the amounts of other organochlorine compounds (other than polychlorinated biphenyls) were not analytically significant. The geometric means of the concentrations of dieldrin and *pp'*-DDE in the shags' eggs for each year from 1964 are shown in Figs. 1 and 2 respectively.

Table 1 Mean (Geometric) Concentrations of Organochlorine Compounds in Shags' Eggs

Year	Farnes			n	Dieldrin Isle of May		n	Overall	
	n	Mean	Confidence limits (95%)		Mean	Confidence limits (95%)		Mean	Confidence limits (95%)
1964	13	0.87	0.63 to 1.19	—	—	—	13	0.87	0.63 to 1.19
1965	54	0.74	0.71 to 0.76	10	0.68	0.43 to 1.09	64	0.73	0.68 to 0.78
1966	44	1.30	1.17 to 1.44	25	0.83	0.74 to 0.94	69	1.11	1.01 to 1.21
1967	52	0.83	0.73 to 0.94	—	—	—	52	0.83	0.73 to 0.94
1968	4	0.18	0.10 to 0.34	5	0.44	0.20 to 0.98	9	0.29	0.19 to 0.46
1969	6	0.68	0.48 to 0.95	11	0.87	0.76 to 0.98	17	0.79	0.69 to 0.92
1970	16	0.45	0.35 to 0.58	38	0.48	0.43 to 0.54	54	0.47	0.42 to 0.53
1971	10	0.28	0.23 to 0.34	14	0.34	0.30 to 0.39	24	0.31	0.28 to 0.35
DDE									
1964	13	0.96	0.67 to 1.37	—	—	—	13	0.96	0.67 to 1.37
1965	54	1.49	1.33 to 1.68	10	1.26	0.81 to 1.97	64	1.46	1.30 to 1.63
1966	44	2.13	1.95 to 2.33	25	2.08	1.84 to 2.33	69	2.11	1.97 to 2.27
1967	52	2.48	2.20 to 2.79	—	—	—	52	2.48	2.20 to 2.79
1968	4	0.55	0.30 to 1.03	5	1.50	0.77 to 2.88	9	0.96	0.63 to 1.48
1969	6	2.06	1.52 to 2.79	11	3.41	2.93 to 3.96	17	2.85	2.39 to 3.40
1970	16	1.91	1.63 to 2.23	38	2.18	1.92 to 2.47	54	2.09	1.90 to 2.31
1971	10	1.10	0.91 to 1.33	14	1.64	1.46 to 1.84	24	1.39	1.22 to 1.58

A short-lived decrease in the concentrations of both dieldrin and *pp'*-DDE was recorded in 1968 with the larger decrease occurring in eggs from the Farne Islands. The fact that the concentrations of both residues were significantly higher in 1967 and 1969 indicates the temporary nature of this change. In 1968 there was an unprecedented mortality of sea-birds, fish and molluscs in the study area, caused by neurotoxins produced by a dinoflagellate bloom (or "red tide"). Further, the inshore ecosystem was temporarily but dramatically altered during this period, the effects being more pronounced around the Farne Islands⁴. It seems probable that the residues in shags' eggs were reduced by the temporary changes in the ecosystem in that year and the possibility of an interaction between the pesticide residues and the neurotoxin produced by the dinoflagellates cannot be excluded.

There is clear evidence that during the period 1964–1971 (excluding the results for 1968), significant changes in the concentrations of dieldrin occurred in the eggs of shags in eastern Britain. A graphical representation (Fig. 1) of the results indicated that the relationship between the concentration of dieldrin in the eggs and the year of collection could be represented by a quadratic function over the period of observation. In order to test this hypothesis the results were fitted by the least squares technique to the relationship

$$\log_{10}y = a + bx + cx^2$$

where y is the concentration of dieldrin in the eggs and x is the time in years (1960 = 0). The fitted relationships (with standard errors in parentheses) are

Farne Islands:

$$\log_{10}y = -0.9929 + 0.3110x - 0.0247x^2$$

(± 0.1772) (± 0.0501) (± 0.0033)

Isle of May:

$$\log_{10}y = -1.6303 + 0.4479x - 0.0314x^2$$

(± 0.3691) (± 0.0990) (± 0.0062)

Combined results:

$$\log_{10}y = -1.0124 + 0.3060x - 0.0236x^2$$

(± 0.1563) (± 0.0434) (± 0.0028)

The coefficients of each of the quadratic terms are negative and statistically significant. The maximum concentration of dieldrin for the combined results is estimated to have occurred

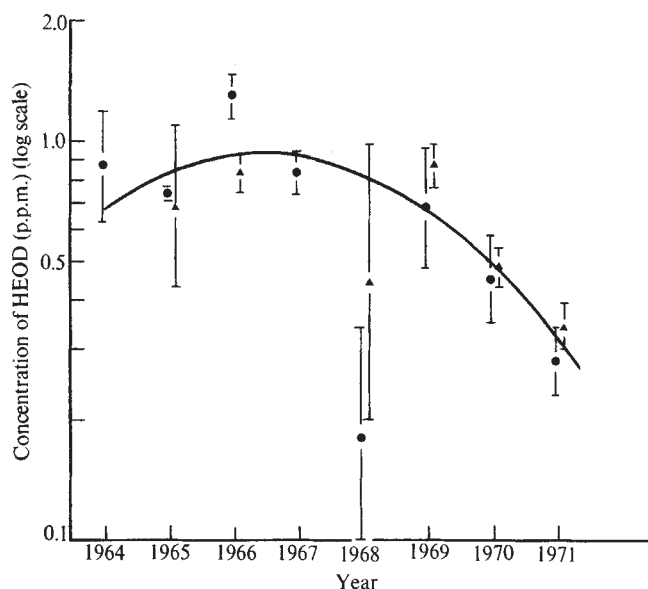


Fig. 1 Concentrations of dieldrin in shags' eggs from 1964 to 1971. The mean (geometric) annual concentrations and confidence limits are indicated by ● (Farne) or ▲ (Isle of May) and the quadratic relationship for the combined results from the Farne Islands and Isle of May as a function of time (1960 = 0 and excluding the results for 1968) is shown as $\log_{10}y = -1.0124 + 0.3060x - 0.0236x^2$.

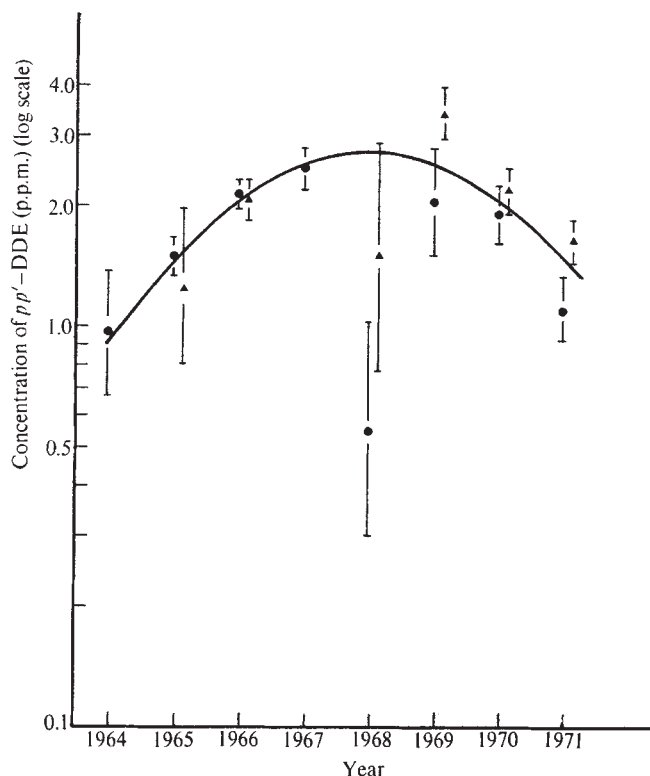


Fig. 2 Concentrations of *pp'*-DDE in shags' eggs from 1964 to 1971. The mean (geometric) annual concentrations and confidence limits are indicated by ● (Farne) or ▲ (Isle of May) and the quadratic relationship for the combined results from the Farne Islands and Isle of May as a function of time (1960 = 0 and excluding the results for 1968) is shown as $\log_{10}z = -1.5029 + 0.4868x - 0.0305x^2$.

in 1966, and a significant decline had started by 1967. Between 1966 and 1971 the average concentration of dieldrin had decreased by about 66%.

Aldrin and dieldrin were first used extensively in Britain in 1956. Some decrease in use occurred with the voluntary restriction on their use as seed-dressing agents for spring-sown cereals in 1962. A further decline in use occurred in 1965 when the inclusion of aldrin in fertilizers was discontinued, and in 1966 when the sale of dieldrin sheep dips was discontinued. Aldrin and dieldrin are still used in Britain, although their uses are now severely restricted. The sales in the British Isles of these products have been declining since 1964; between 1964 and 1967 there was a decrease of about 60% in the sales of these compounds, and by 1970 the decrease was about 69% (data supplied by Shell International Chemical Co.).

The area under study does not constitute a closed system as dieldrin was still entering the system, albeit probably on a reduced scale, and there may also have been losses from the North Sea area into the North Atlantic. It is of interest, however, that the concentrations of dieldrin in the kidney fat of sheep reared in Britain showed a very rapid decline between 1964 and 1969, the mean concentration being approximately halved each year between 1965 and 1969⁷. The concentration of dieldrin in the adipose tissue of members of the general population in Britain also seems to have been declining in recent years, and the mean concentration of this compound in whole prepared meals for humans also decreased between 1965 and 1967⁸⁻¹⁰. The declines of the dieldrin concentrations in the latter two types of samples, like that of dieldrin in the shags' eggs, are relatively slow. A more rapid decline, although somewhat slower than that of the concentration of dieldrin in mutton fat in Britain, of the concentration of dieldrin in the eggs of the golden eagle (*Aquila chrysaetos*) in Scotland, has been reported¹¹. The precise rate of decrease is difficult to

measure, however, because the data were collected over a relatively short period of time and samples were taken from different parts of Scotland.

The indications are that the restrictions on the use of aldrin and dieldrin in the British Isles have resulted in a decrease of coastal contamination by HEOD in eastern Britain.

The concentrations of *pp'*-DDE in the eggs of shags have also changed significantly during the period under study (Fig. 2). The quadratic relationships (excluding the 1968 results) for this compound are

Farne Islands:

$$\log_{10} z = -1.4862 + 0.4931x - 0.0320x^2$$

$$(\pm 0.1719) \quad (\pm 0.0486) \quad (\pm 0.0032)$$

Isle of May:

$$\log_{10} z = -2.1193 + 0.6418x - 0.0393x^2$$

$$(\pm 0.3739) \quad (\pm 0.1003) \quad (\pm 0.0063)$$

Combined results:

$$\log_{10} z = -1.5029 + 0.4868x - 0.0305x^2$$

$$(\pm 0.1547) \quad (\pm 0.0429) \quad (\pm 0.0028)$$

where *z* is the concentration of *pp'*-DDE and *x* is the time in years (1960 = 0).

The coefficients of the quadratic terms, as in the case of dieldrin, are negative and statistically significant. The maximum concentration is estimated to have occurred in 1967–1968, and a significant decline had occurred by 1969. Between 1968 and 1971 the mean concentration of *pp'*-DDE decreased by about 47%.

DDT has continued to be manufactured and used in large amounts but we have failed to find information on the quantities of DDT used in Britain in recent years. The recently reported similarity in effects on living tissues of polychlorinated biphenyls and DDT-type compounds suggests that further work on the fate of all these organochlorine compounds in the environment is required. The changes in the concentrations of these two organochlorine compounds in the eggs of shags are of particular interest in relation to attempts to establish mathematical models for the behaviour of such compounds in ecosystems.

The significant declines of the concentrations in recent years are particularly noteworthy as the shag is a member of the fourth trophic level, and the decline of the concentration of *pp'*-DDE is inconsistent with the model prepared by Harrison *et al.*¹² which predicts no decrease in DDT concentration in animals at the top of the trophic structure even if no further quantities of this substance are added to the biosphere.

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Eradicating Mosquitoes using Translocations: a First Field Experiment

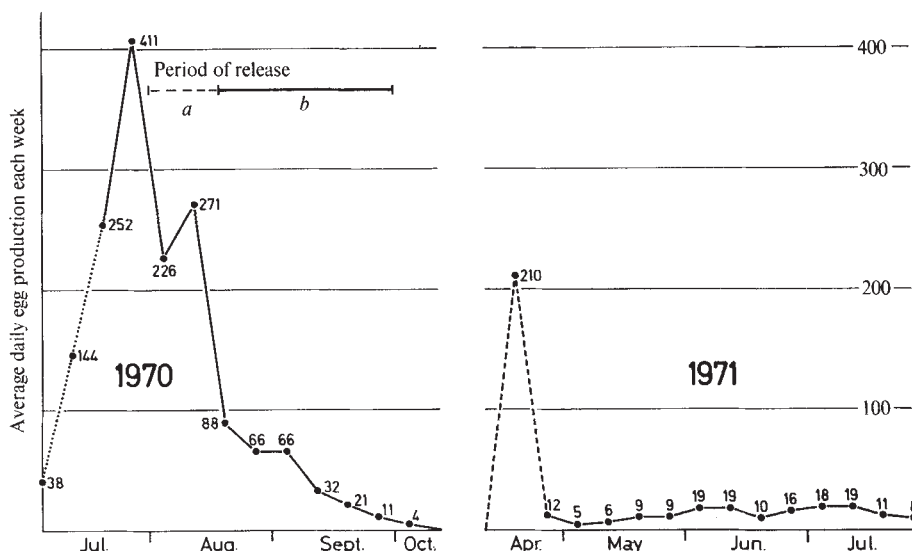
THEORETICAL considerations have led to the assumption that chromosomal translocations with ensuing semisterility could be used to control pests¹⁻³. Inversions could have the same effect in animals and plants in which crossing over occurs in both sexes⁴. Many translocations of different type and with various degrees of sterility have been produced in the mosquito *Culex pipiens*⁵⁻⁸. Preliminary laboratory experiments with these translocations have shown that a cage population can be diminished and finally exterminated after a few generations by the release of translocation heterozygotes into the population⁹.

In 1970 the translocation system was tested against *Culex pipiens* in the field. The more or less isolated village of Notre Dame near Montpellier (southern France) was chosen for the experiment because the only breeding place there, a large closed well, allowed accurate daily counting of adult mosquitoes which were produced there. Production started in late June and early July 1970, with a few hundred insects per day, and went up to more than 20,000 in mid-August. Releases of males heterozygous for a sex linked translocation with approximately 50% sterility started on August 1. For the first 2 weeks the number of released males was much lower than the number of males born in the well. Only from August 18 onwards did the ratio of released males to wild males increase to 1:1. It could then be enhanced to an average of 5:1 during the remaining release period which ended on September 30.

To evaluate the influence of the translocation males on the population, three water containers, each with a surface of about 1 m², were placed around the top of the well on July 22. All egg rafts laid on these containers were inspected for semisterility. By August 6, 5 days after the first release (Fig. 1), the first egg rafts with semisterility were found. The percentage of such rafts increased steadily to about 70% early in September, reaching 95% at the end of September. Parallel with the increase of egg rafts with reduced hatching, the daily production of adults went steadily down from a peak of more than 20,000 on August 12 to little more than 100 at the end of September. A more detailed account of the observations in 1970 has been published elsewhere¹⁰.

Observations continued in 1971, but no more releases were made. The exitus of hibernating females was earlier this year. On April 15 and 17 a total of 420 egg rafts were found on the sampling containers (Fig. 1), 365 of them (89%) with translocation sterility. This shows that the translocation had persisted during the winter. The average number of rafts was extremely low during May and increased only about two-fold in June and July. A comparison of daily egg raft production in 1971 with that at corresponding times in 1970 shows that the production in 1971 was very much lower. There is no doubt that the reduction of the population size reflected in the lower egg raft production must be due in the first place to the influence of the introduced semisterility. The breeding place and its production potential had in no way changed during the 2 yr. As a deep, subterranean well it remains almost unaffected by changes in the surroundings. Nevertheless, the extremely low production cannot be explained only with the influence of the inherited semisterility. Higher predator and parasite pressure has probably depressed the already diminished population.

Fig. 1 Egg raft production per week on water containers in the vicinity of an isolated breeding place of *Culex pipiens* in the village of Notre Dame in 1970 and 1971. The dotted line at the beginning of the curve for 1970 indicates number of egg rafts calculated from adult production, but not actually observed. The broken line at the beginning of the curve for 1971 indicates irregular observations. The period of releases is divided into (a) with insufficient numbers and (b) with sufficient numbers of males released.



It would have been very easy to eradicate the remaining survivors of this formerly flourishing population by a new round of releases, perhaps by the incompatibility principle¹¹. But this experiment was only intended to demonstrate that translocation semisterility can diminish a natural population, which is obvious from the data presented here. The genetic system used here had depressed the population to a much lower level. The surviving population may, however, increase somewhat in the future, when the predator and parasite pressure can be expected to have become weaker.

The diminution of pest insect populations with inherited semisterility can be anticipated to be effective for most pests. This type of birth control has, besides its cheapness, the advantage that it allows for the reduced productivity of a species to persist, but at a level which is tolerable for economic, public health or other reasons.

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Diaseris distorta, an "Acrobatic" Coral

Diaseris distorta (Michelin), a free living scleractinian coral, is a common fungiid in muddy sands such as those on the leeward side of Lizard Island in the Australian Great Barrier Reef (Australian Survey Map SD 55.9, grid reference 33143). Goreau and Yonge¹ collected the specimens here described from this locality, in 12 m of water, where densities reached a thousand per square metre. In these clones most of the corals were less than 10 mm in diameter. Eighteen months

later, when their activities were investigated, these specimens, less than 10 mm in diameter, were still active in the open seawater aquarium at Discovery Bay Marine Laboratory, Jamaica.

Fungiid corals have long had an almost mythical reputation for their mobility, for apart from their ability to turn themselves over when inverted, and their capacity for sediment rejection by rising through it¹⁻⁵, there seems to be no record of their independent locomotion. But the twelve specimens of *Diaseris* kept in the open seawater system aquarium in Jamaica performed voluntary "acrobatics". Wandering corals which had escaped from their containers were often seen. At first these were thought to be displaced by variations in current strength, but an accidental observation proved that this was not the case. One of the corals was seen to start to move (Fig. 1): its "anterior" tentacles—that is, tentacles which were at the "front" when the coral started to move, reached forward in a rampant manner—and its "posterior" tentacles retracted. Ten minutes later it had developed a mucous float posteriorly. This appeared to be coming from the aboral surface, and it incorporated many minute sediment particles and even more small gas bubbles (Fig. 1 (2)). The coral then staggered across the dish in a series of jerky moves, its "front" tentacles reaching forward to grab the substrate and its "posterior" bouncing on its gas-filled mucous float. Then *Diaseris* slowly mounted the dish (Fig. 1 (3); Fig. 2) where it seemed to spend an interminable time (just under 50 min) rocking to and fro before climbing off. It made several attempts to descend by rocking itself into a position at an angle to the edge. Ultimately it reached an angle of more than 60° inclination to the vertical wall, whereupon it suddenly loosened itself as though adhesion had ceased. The coral then fell, oral surface down, on the sediment 5 mm below (Fig. 1 (7); Fig. 2 (7)). Thereupon the polyp shrank and lay motionless for a while. But during the next hour it moved down slope on its oral surface with its mucous float still intact but this time in advance of it. Ten minutes later, having travelled 30 mm on its oral surface, the coral set about righting itself. This action involved a series of movements combining distension, during which the sediment-touching tentacles on one side were visibly grasping at the substrate, while at the opposite side the mucous float was increasing in size. As before, the polyp rocked to and fro until it succeeded in overbalancing and thereby righting itself. It then moved back up slope in the same furrow it had made during its earlier traverse. Thus in 5 h and 20 min the coral had travelled 90 mm, or thirteen times its skeletal diameter, at an average speed of 0.3 mm/min and a top speed of 3 mm/min.

To determine what was happening during righting activities, another specimen was inverted in a sample of the same muddy sand and observed through a stereo-binocular microscope at a

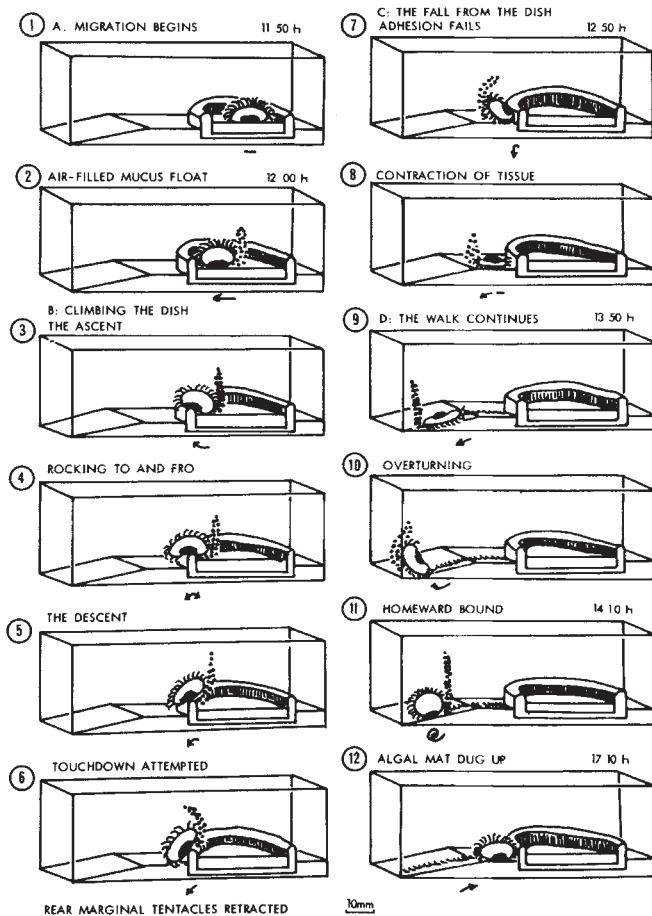


Fig. 1 A series of block diagrams showing *Diaseris distorta* (Michelin) undertaking independent movement between 1150 and 1710 h.

magnification of $\times 20$ (Fig. 3). When the polyp was inverted it contracted its tissues (Fig. 3 (1)). The polyp then began to swell until it reached an asymmetrical kidney bean shape (Fig. 3 (2-4)). On reaching its maximum asymmetrical extension the polyp contracted. Simultaneously it ejected a jet of water at the substrate from the stomodeum (Fig. 3 (4-6)). This resulted in the formation of a small deflationary hollow on the stomodeal side of the coral. The polyp retracted to its minimum size, redistended and repeated the process thereby deepening the depression (Fig. 3 (6-7)). Then the polyp swelled up to its maximum size and, with its disk at right angles to the substrate, rocked to and fro on its "posterior" tentacles (Fig. 3 (8)). Once more it retracted and repeated the exercise until it had edged itself into its newly constructed hollow, whereupon it collapsed (Fig. 3 (5-11)). After a short pause it began to swell and rock itself into a vertical position (Fig. 3 (12-13)), where it tottered for a few seconds, before it finally fell oral surface uppermost (Fig. 3 (15)).

This experiment was repeated three times with similar sized animals. On each occasion the same sequence of events was followed. But some corals proved to be quicker than others. Thus the time taken for the whole manoeuvre varied from 20 min to 3 h and appears to be quite unpredictable.

The coral was buried, the right way up (that is, oral surface uppermost), in a sample of the sediment on which it had been living. It was watched extracting itself. Once again this was viewed through a stereo-binocular microscope ($\times 20$) and the sequence of events is recorded in Fig. 4. In this experiment *Diaseris* followed exactly the same behavioural pattern of sediment rejection as *Manicina* (*Maeandra*) *areolata* (Linnaeus) which has been recorded on several occasions (D. F. Squires, unpublished manuscript, Smithsonian Institution and refs. 6 and 7). The polyp distended itself by the stomodeal

uptake of water and used this technique to move the unwanted sedimentary particles at the same time as it rose through the sediment by reducing its relative density with respect to the surrounding sediment. Its tentacles were used as a tertiary mechanism for freeing its upper surface and removing unwanted particles from its oral disk. Several experiments showed that *Diaseris* is as varied in its speed of rejecting sediment as it is at turning over.

Diaseris distorta demonstrates that certain scleractinian corals are considerably more active than had been appreciated. Moreover, in competition with its free living fellow fungiids, *Fungia actiniformis* variety *palawensis* Döderlein (Abe³) and *Fungia scutaria* Lamarck, *Diaseris* fares well. Of these, only *Diaseris* overturned voluntarily when placed oral surface downwards; and it invariably turned over in a matter of hours if not minutes. But this property, even if most conspicuously developed among the fungiids, is not restricted to this family, for the faviid *Manicina* (Fabricius⁷) is also capable of similar movement, though it periodically remains inverted for a matter of weeks. Moreover, overturning is not limited to free living scleractinians, for experiments with two common rock-attached musciid corals, *Isophyllia* and *Isopyllastrea*, showed that specimens of both genera overturned in a matter of hours.

In all these cases overturning was effected by a common process: controlled manipulation of the hydrostatic skeleton following distension of the polypal tissues by the stomodeal

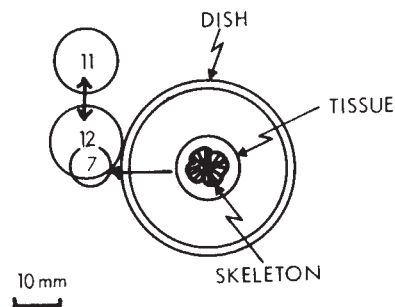


Fig. 2 Plan of movement illustrated in Fig. 1. The coral is drawn to scale with its environment. The stages marked correspond to those illustrated in the block diagrams.

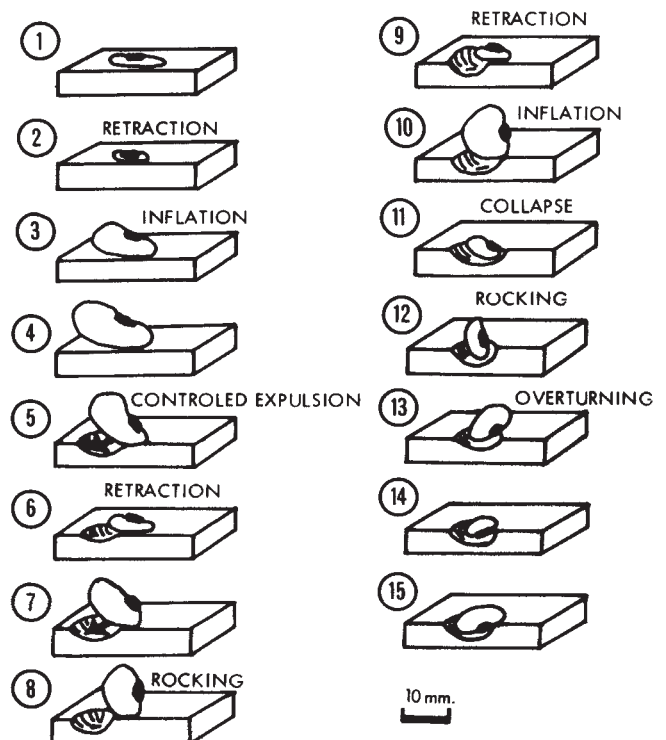


Fig. 3 The righting reactions of *D. distorta*.

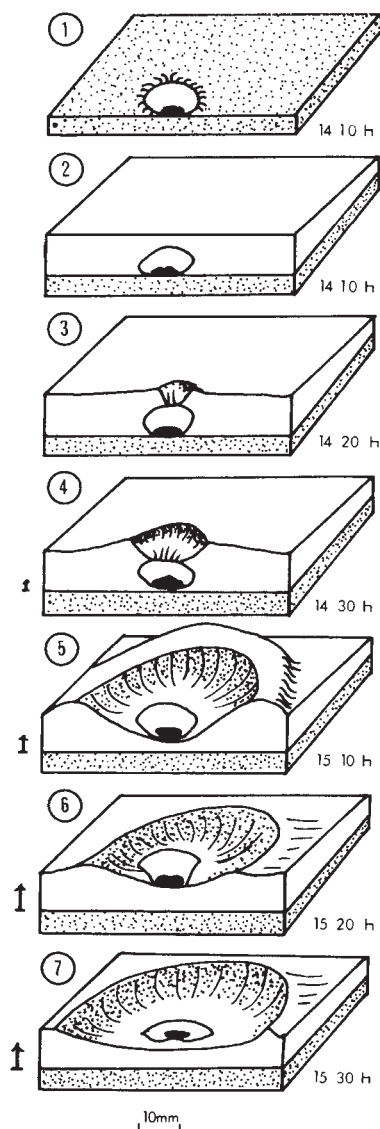


Fig. 4 Block diagrams illustrating *D. distorta* extracting itself from an inundation of sediment. The arrow indicates the amount the polyp raises itself with respect to its original substrate at various stages.

uptake of water. This behavioural feature has been recognized among other coelenterates, for example, the hydroid *Hydra* and the sea anemone *Metridium senile*^{9,10}, but is less well known among scleractinians^{3,6}. Cinefilmed experiments on sediment rejection by twenty-one scleractinian corals selected from a range of environments in the Florida reef tract, however (ref. 11), indicate that distension of the polyp averages 300–400% over size when contracted, which equates with that observed in the sea anemone *Metridium*⁹. In the case of the scleractinians studied the efficiency of the mechanism can be deduced from a study of the calical surface area and ornamentation. The more active genera have the greater surface area and the more complex septal ornament. Thus, among the non-reef fungiids, *Diastrea* has a lighter skeleton with more complex septal ornament of tubercles, synapticulae and perforate septa, whereas the less active *Fungia scutaria* has a heavier imperforate skeleton with solid synapticular bars. Similarly, of the reef corals tested, *Porites*, with its remarkably perforate skeleton, exhibits a propensity for removing sediment from its oral surfaces by actively pushing the grains away with its tentacles; whereas, in contrast, the more passive sediment rejectors, which are largely dependent on ciliary action and possess only limited powers of distension (for example, *Acropora*, *Oculina*, *Agaricia*, *Eusmilia*), have smoother septa,

a more limited septal surface area, less calical relief, and imperforate septa. Moreover, this last group of corals is restricted to moving only fine particles (<250 μ m), whereas the others are additionally capable of shedding larger particles (<2,000 μ m). Thus observations on *Diastrea* and comparative studies of selected scleractinian corals lead to the conclusion that these corals are predictably size specific in their capacity for removing sediment.

I thank the many institutions and persons who have assisted me, especially the late Professor T. F. Goreau.

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Vibration Perception by Chaetognaths

THE manner in which chaetognaths feed deserves careful examination because they are abundant and cosmopolitan planktonic predators. When these animals feed, they hang motionlessly and then dart at prey which swim near. The exact means by which they perceive or locate prey around them is only partially understood. I wish to present evidence for their use of a vibrational rather than a visual perception system.

Chaetognaths can feed successfully without light so the visual perception of prey is probably unnecessary. In nature their predation is not restricted to daytime when light is brightest; in fact, they probably feed mostly at night. In darkened laboratory conditions *Sagitta setosa*¹ and *S. hispida*² will feed on non-bioluminescent forms. The absence of pigment in the eyes of several species³ also illustrates the relative unimportance of visual perception to these animals.

In contrast, the chaetognaths' ability to catch food organisms is related closely to the activity of the prey. *S. hispida* readily consumes *Artemia* that are actively swimming but not the eggs of the latter when swirled in water⁴. *S. hispida* feeds well on live plankton but not on the same material which first is killed by a quick thermal change⁴. These observations indicate that, in order for chaetognaths to locate and attack prey, the prey must be actively moving—that is, producing vibrations and turbulence.

The importance of vibrations is further demonstrated by the response of chaetognaths to inanimate vibrating objects. In a series of experiments with different probes and illumination, Horridge and Boulton⁵ found that *Spadella cephaloptera* will attack probes which are vibrating. The attack response occurs when the probes are vibrating at frequencies between only 9 and 20 Hz; a frequency of 11 Hz elicits the maximum percentage of responses. When similar experiments were performed with *Sagitta elegans*⁶, slightly different results were obtained. An escape response was observed with all of the following stimuli: vibration of probes at high amplitude, disturbances of the surface film, currents and touch. The results indicate that the experimental animals were responding to only large-scale turbulence. The series of experiments with

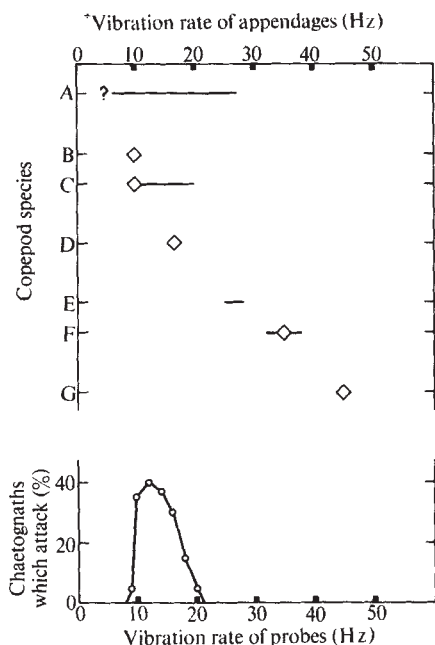


Fig. 1 Vibration rate of copepod appendages and the response of chaetognaths to probes vibrating at the same rate. Upper section shows rates for the copepods listed in Table 1. (Normal rate, diamond; probable range of rates, line.) Lower section shows the percentage of *Spadella cephaloptera* which attack a probe vibrating at various frequencies (redrawn from data by Horridge and Boulton⁵).

both species indicate that chaetognaths locate and attack organisms probably by the perception of vibrations rather than gross turbulence.

In previous studies the significance of the frequency range over which the attack response of the chaetognath can be elicited has not been considered. As described, *Spadella cephaloptera* attacks probes when they are vibrating between only 9 and 20 Hz. What are the reasons for this specific response curve?—that is, the limited frequency range over which the response occurs and the maximum percentage of responses to a particular frequency? To answer this, it is necessary to study the results from another field of research.

The chaetognaths' principal food organisms, copepods, create vibrations when they beat their appendages to filter feed and swim. The filter feeding process has been studied often with stroboscopic techniques in order to measure the frequency at which the appendages vibrate. The studies have determined that species have characteristic frequencies. Listed here in Table 1 and graphed in Fig. 1 are the frequencies for several copepods that live in the same waters as *Spadella cephaloptera*. Also shown in Fig. 1 is the percentage of *S. cephaloptera* which respond to probes vibrating at various frequencies.

The overall range of vibrations produced by the copepods corresponds with the range of vibrations to which the chaetognath responds. This relationship indicates that the attack response of *S. cephaloptera* would be provoked by the copepods. Furthermore, the range and mode of the chaetognath's response curve correspond precisely with the range and normal frequency produced by one copepod: *Calanus finmarchicus*, s. lat. (rates B and C in Fig. 1). This indicates that *S. cephaloptera* may select preferentially *C. finmarchicus* by its characteristic frequency.

The relation of the data on copepods clarifies two important points. One is that chaetognaths can locate and attack at least copepods by vibration perception. This method of perception is entirely compatible with their behaviour: chaetognaths wait motionlessly for prey and therefore create no interfering noise themselves, whereas copepods create vibrations continuously when swimming and filter feeding. The second point is that chaetognaths may select specific copepods by the frequency the latter produce. This explains

Table 1 Vibration Rates of the Appendages of Copepods when Filter Feeding and Swimming

Copepod species	Vibration rates	Line in Fig. 1
<i>Centropages typicus</i>	Maximum rate 27.3 Hz, but normal rate rather below this figure ⁷	A
<i>Calanus finmarchicus</i> , s. lat.	About 10 Hz ⁸	B
<i>Calanus finmarchicus</i> , s. lat.	Estimated rate about 10 Hz, but up to 20 Hz ⁷	C
<i>Anomalocera patersoni</i>	About 16.7 Hz ⁷	D
<i>Temora longicornis</i>	25 to 28.1 Hz ⁷	E
<i>Temora longicornis</i>	35 ± 3 Hz ⁶	F
<i>Eurytemora velox</i>	About 45 Hz ⁷	G

All of the vibration rates except F were measured with a variable-speed stroboscope. The pulsing light of the stroboscope was used to "suspend" the cyclic movements of the appendages, and the rates were read from the stroboscope. Rate F was measured by visual correlation with a vibrating probe.

the specific response curve obtained for *S. cephaloptera*. There is need for additional experimental work on this aspect, principally on the ranges of frequencies produced by copepods and the specific response curves of other chaetognaths. With this information it may be possible to distinguish food chains by the correlation of the vibrations produced by copepods and of the vibrations perceived by chaetognaths. In the future it may also be possible to distinguish different feeding patterns for chaetognaths: generalists with relatively wide response curves or specialists with relatively narrow ones, like *S. cephaloptera*'s.

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Antagonism between Indoleacetic Acid and Abscissic Acid on a Rapid Phytochrome-mediated Process

THE earliest known physiological response brought about by phytochrome after absorption of red photons is a change in the electrical charge on cell surfaces that causes, in suitable conditions, a rapid attachment of root tips of barley and mung beans to glass charged with phosphates¹⁻⁴. Far-red irradiation results in detachment of the root tips. I have shown previously that in the presence of greater than 10⁻⁸ M indoleacetic acid (IAA), mung bean root tips fail to attach following red irradiation³. This indicates that IAA is able to overcome the influence of phytochrome. I shall show here that in contrast to IAA, abscissic acid (ABA) at very low concentrations causes mung bean root tips to adhere to glass after successive red and far-red irradiation.

The methods and materials used will be published in detail in another journal. In brief, mung bean seeds (*Phaseolus aureus*, variety Oklahoma 612), steeped in water for 8 h, were exposed to 10 min of far-red light (190 μW cm⁻² at 700 to 750 nm). They were then placed in darkness to germinate

over aerated deionized water at 23° C. After 3 days, ten root tips of 2 mm length were excised from the seedlings and placed in a 'Pyrex' beaker containing 10 ml. of solution of the following composition: KCl, MgCl₂, and CaCl₂ of 10⁻⁴ M; MnCl₂, 2 × 10⁻⁶ M; L-ascorbic acid, 10⁻⁶ M; and 3-indoleacetic acid, 2 × 10⁻¹⁰ M. Various amounts of abscisic acid (supplied by Dr J. C. Bauernfeind, Hoffmann-La Roche) were added to this solution. The beaker was placed immediately on a gramophone turntable in a beam of red light (4.0 μW cm⁻², 650 to 680 nm), and the root tips were arranged 2.5 cm from the centre of the turntable. After 1 min of red irradiation, the turntable was gradually accelerated to a speed of 33 r.p.m. for a few revolutions. The number of root tips remaining attached was counted while the turntable was gradually slowed down. This procedure of scoring once every minute was carried out for 5 min in red light and for 7 min in far-red light (8.9 μW cm⁻², 710 to 750 nm). After each run, the beaker was washed with a detergent containing phosphates. All operations were done in very dim light and at 23° C.

Some typical results are presented in Figs. 1 and 2. Far-red irradiation of mung bean seeds produces seedlings with root tips showing poor attachment to glass in red light (Fig. 1a). (Red irradiation of seeds results in seedlings with root tips which attach strongly in red light.) Addition of ABA to the solution to a concentration of 2 × 10⁻¹⁰ M quickly enhances root tip attachment in red light (Fig. 1b), but in far-red light the root tips gradually detach with time. When the ABA concentration is increased to 5 × 10⁻¹⁰ M, however, the root tips remain attached in both red and far-red light (Fig. 2a). This effect of ABA on attachment is soon reversed by increasing the IAA concentration to 2 × 10⁻⁹ M (Fig. 2b). In a more detailed publication, data will be presented which show that the inhibitory effect of IAA on attachment can be reversed by ABA.

From the results presented, it seems that phytochrome acts in conjunction with IAA and ABA to bring about fast changes in surface charges and that these hormones have opposing effects on the surface potentials: ABA inducing a positive and IAA inducing a negative potential.

When the endogenous concentrations of IAA and ABA are at suitable levels, phytochrome on absorbing red photons could increase the concentration of ABA relative to that of IAA, thereby bringing about a positive membrane potential which causes adhesion of root tips to the negatively-charged glass surface. Absorption of far-red photons could evoke phytochrome to increase the concentration of IAA relative to that of ABA which then produces a more negative membrane potential that leads to root tip detachment. Etherton⁵ has reported that low concentrations of IAA induce a more negative membrane potential in oat coleoptile cells. Such a scheme would be in line with the observation that when IAA is in

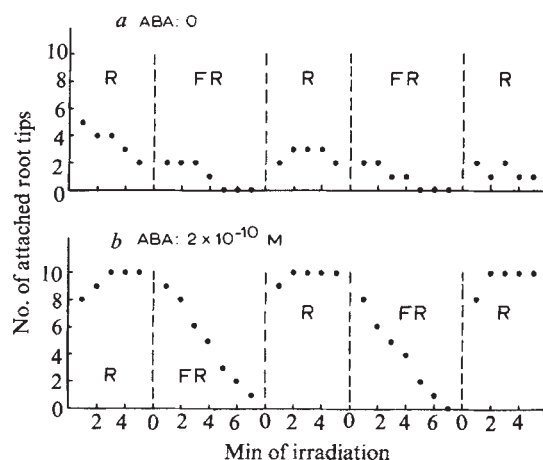


Fig. 1 Effect of ABA at a concentration of 2 × 10⁻¹⁰ M on attachment of mung bean root tips on glass in red (R) and far-red light (FR).

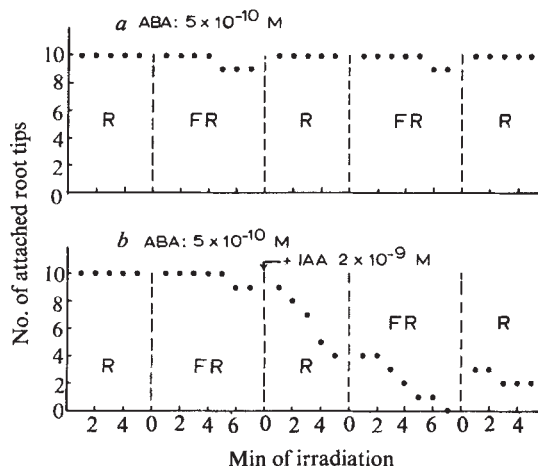


Fig. 2 ABA-induced attachment of mung bean root tips on glass in red (R) and far-red light (FR) and its reversal by IAA. IAA was added at the time indicated.

excess, the root tips fail to attach in both red and far-red light; and when ABA is in excess, the root tips remain attached in both far-red and red light.

Since the photoreversible surface charge is quickly affected by very low concentrations of IAA and ABA, the initial sites of action of these plant hormones are probably on the plasma-membrane. Moreover, the number of these sites on the cell membrane is most likely to be few. Although no competition studies have been made, the rapid reversal of ABA effects by IAA and *vice versa* suggests the possibility that both hormones compete for the same sites on the membrane.

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Competitive Exclusion in Forest Trees

AYALA¹ demonstrated experimentally that two species of *Drosophila* competing for limited food and space can coexist. Based on this evidence, he rejected the principle of competitive exclusion (Gause's principle), which states that two species cannot coexist indefinitely if they compete for the same limited resources; that is, if they occupy the same ecological niche. He suggests that competitive coexistence in *Drosophila* is allowed by (1) differences between the two species in competitive fitness of the larval and adult stages, and (2) the inverse dependency of each species' competitive ability on its relative frequency or population density². In his two articles, Ayala reviews and cites the literature pertaining to competitive exclusion in animals.

The relationship of birth and death rates to population density recently has been documented for several tree species in the beech-birch-maple forest cover type of New England³. Working with two of these species, yellow birch (*Betula alleghaniensis* Britton) and paper birch (*B. papyrifera* Marsh.), I can show that rate of increase or competitive ability in both species tends to be inversely related to total population density. The birth-death patterns, however, are such that the principle of competitive exclusion will necessarily operate in a predictable

direction. These findings in no way contradict Ayala's well founded conclusions. They simply illustrate that, in forest trees, population-density relations may force rather than negate the principle of competitive exclusion.

Approximate birth and death rates for yellow and paper birch over total population density (made up of several tree species) were developed from data previously published³ covering a 16 yr remeasurement period (the second remeasurement period) in second-growth beech-birch-maple stands. The curves represent a second degree polynomial fitted to observations at three density levels (Fig. 1) ranging from an average of about 210 to 270 stems/acre (0.405 hectare). Birth rate (B) is the annual probability that a parent tree will produce an established offspring, defined as a tree at least 4.5 inches in diameter. Death rate (D) is the annual probability that a tree will die.

At this point, I can reasonably assume that individuals from different species have the same inhibitory effect. Inhibition among northern hardwood species in general, and among birch species in particular, is known to involve physical competition for light⁴. Thus, the inter- and intra-species effect of an individual should not differ appreciably. No evidence indicates that these two birches differ markedly in inhibitory effects. Paper birch apparently has a somewhat less dense crown, but also grows somewhat faster than yellow birch.

Such birth-death curves for forest tree species are variable in nature. The curves shown here, however, exhibit logical, well accepted characteristics. Birth rates decline at high population densities. Death rates tend to increase or remain roughly steady as population density increases. Both species exhibit compensation points where the birth and death curves intersect, and the upper compensation point for yellow birch occurs at a higher population density than that of paper birch, a result of the former's greater shade tolerance. (I am not sure that the lower compensation point for yellow birch is truly representative of the species.)

I use birth-death curves as defined above to simulate population growth and successional change. Taking each mature tree in turn, the probability that this tree will lead to an annual increase, decrease, or no change in population size is

$$P(\text{increase}) = B(1.0 - D)$$

$$P(\text{decrease}) = D(1.0 - B)$$

$$P(\text{no change}) = (B \times D) + (1.0 - B)(1.0 - D)$$

Beginning with two hypothetical populations of yellow and paper birch, one containing 260 stems and the other 230 stems per acre, future population changes were simulated by computer. The probabilities defined above and the birth-death curves in Fig. 1 were used to predict the addition and demise of individual stems (Fig. 2).

For both populations, simulated population size stabilized at 250 to 255 stems. In both cases, yellow birch steadily

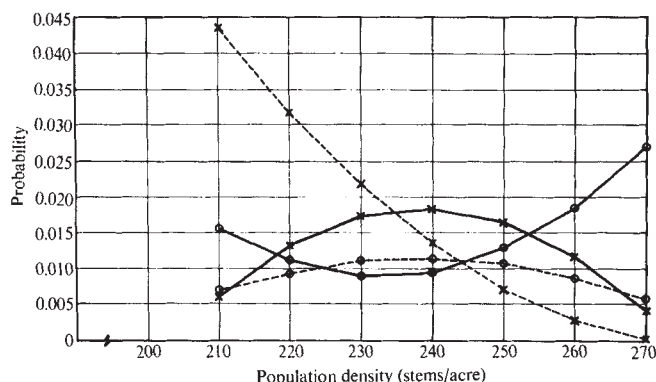


Fig. 1 Annual birth (x) and death (o) rates (probabilities) over total population density for yellow (—) and paper (---) birch.

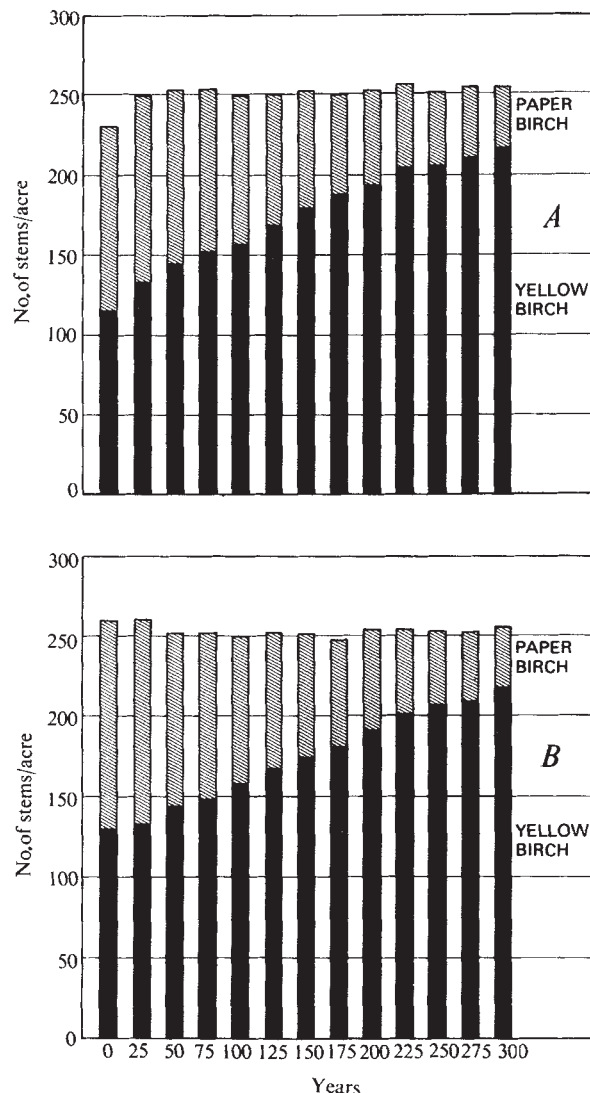


Fig. 2 Simulated numbers (averages of five runs) of paper birch and yellow birch stems over time beginning with (A) 115 stems/species, and (B) 130 stems/species.

displaced paper birch. Indications are that this trend will continue.

An examination of Fig. 1 shows why the principle of competitive exclusion must operate. Beginning with a population of 230 stems, both species will tend to increase until the compensation point for paper birch is reached at about 244 stems. Beyond that point, yellow birch will continue to increase while paper birch will decline. The population eventually will stabilize at about 254 stems, all composed of yellow birch.

Beginning at the higher population density of 260 stems, the population will decrease rapidly and drop below the compensation point for yellow birch. Then yellow birch will begin to increase, while paper birch will continue to decline. Stability will occur again at 254 stems, all of yellow birch.

Where tree species exhibit birth-death patterns similar to the birches, the species whose compensation point occurs at the higher density must eventually predominate.

Unlike *Drosophila*, competitive exclusion in forest trees cannot be investigated experimentally in the laboratory. Last summer, however, two colleagues and I had the opportunity to do some plot work in "The Bowl", a 206 hectare (510 acre) Federal Research Natural Area in the White Mountain National Forest, New Hampshire, which contains some of the last remnants (about 39 hectares) of virgin beech-birch-maple forest. These virgin stands have never been cut or burned to my knowledge. In those portions of virgin hard-

wood forest that had not experienced heavy windthrow, we found both young and old yellow birch trees along with beech (*Fagus grandifolia* Ehrh.) and sugar maple (*Acer saccharum* Marsh.), but hardly any paper birch. (In two examinations of the beech–birch–maple portion, I found one dead paper birch as well as one live, mature specimen overhanging the brook that bisects "The Bowl".) Paper birch is prevalent in surrounding stands. Apparently, competitive exclusion between paper birch and the few remaining species had nearly run its course.

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Heterotrophy by Plankton in Three Lakes of Different Productivity

THE utilization of dissolved organic compounds by heterotrophic microorganisms is an important process in the aquatic carbon cycle and in the functioning of aquatic ecosystems in general¹. It has been hypothesized that the only significant uptake of naturally occurring dissolved organic compounds by plankton is carried out by heterotrophic bacteria and that competition by phytoplankton for these compounds is insignificant². In this communication I present evidence which does not support this hypothesis and I also show the importance of heterotrophy in relation to the total metabolism of plankton communities in freshwater ecosystems.

The basic technique used in this study was the carbon-14

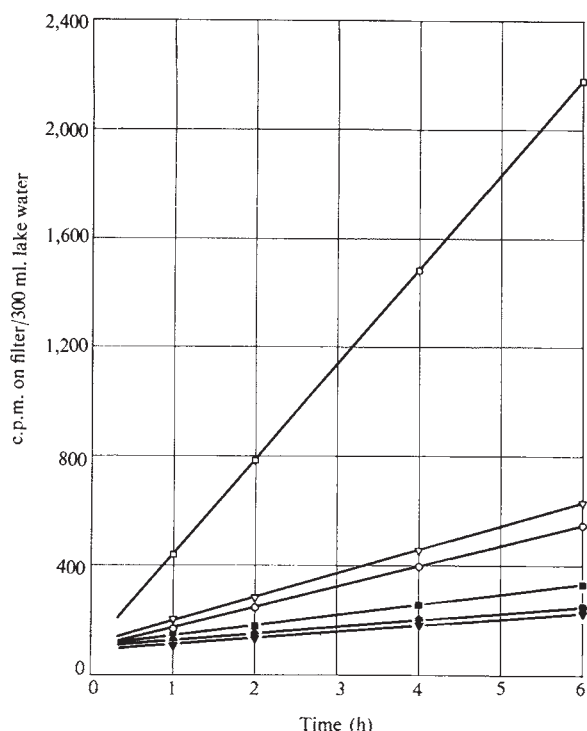


Fig. 1 Uptake of $^{35}\text{SO}_4^{2-}$ by plankton from three lakes of different productivity. $\circ$, Oligotrophic system; ∇ , mesotrophic system; $\square$, eutrophic system. Open symbols, incubation in light; closed symbols, incubation in dark.

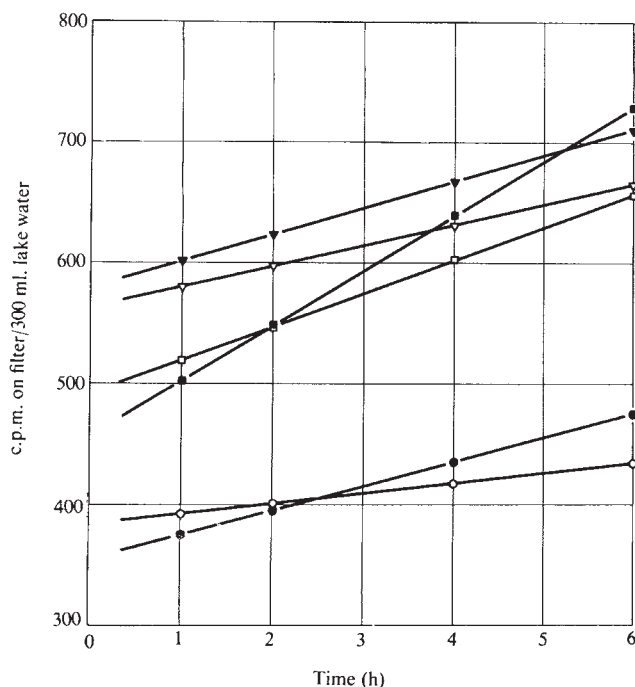


Fig. 2 Uptake of a heterogeneous mixture of ^{35}S -labelled organic compounds by plankton from three lakes of different productivity. Symbols as in Fig. 1.

method of measuring primary productivity³, with a heterogeneous mixture of dissolved organic compounds labelled with sulphur-35 used as the isotope tracer instead of $^{14}\text{CO}_2$. This organic tracer was produced in the laboratory by adding $\text{H}_2^{35}\text{SO}_4$ (1 mCi) to a flask containing 1 l. of water from a eutrophic pond. This culture was incubated under light (3,000 foot candles) at 25° C with constant stirring for 65 h. Labelled dissolved organic compounds were extracted from this culture by: (1) breaking the cells by sonication; (2) removing particulate matter by filtering through a 0.45 μm membrane filter; (3) passing the filtrate through an anion exchange column of 'Dowex 1-X8' (50–100 mesh) to remove inorganic SO_4^{2-} . The effluent from the column contained organic sulphur at 1,200 c.p.m./ml. Tests for SO_4^{2-} were run on the column effluent. After the extraction procedure, the column was charged with ten volumes of a saturated solution of Na_2SO_4 and then that effluent was tested for SO_4^{2-} . These tests showed that no inorganic sulphur had passed through the column.

Rates of uptake of labelled materials by plankton were determined during June and July 1969 for a eutrophic, a mesotrophic and an oligotrophic system. The organic tracer was used at 2.0 ml. per 300 ml. BOD bottle. The uptake rates of inorganic sulphate ($^{35}\text{SO}_4^{2-}$) were also determined, at 4 μCi per bottle. Transparent and opaque bottles containing lake water were inoculated with isotope and were incubated on a platform floating at the 15 cm depth from which the water was collected. At intervals of 1, 2, 4 and 6 h after inoculation the contents of four transparent and four opaque bottles from each isotope treatment were passed through 0.45 μm membrane filters, which were then washed, dried and counted for radioactivity. Data were fitted to a linear regression by least squares and regression slopes were compared by t test⁴.

Plankton incubated in light took up inorganic isotope at a higher rate than those incubated in the dark (Fig. 1). In all three lake systems, however, organic isotope was taken up faster by plankton incubated in the dark ($P < 0.05$) (Fig. 2). There were no differences in the levels of filterable isotope, after incubation in the light or dark, of filter-sterilized lake water inoculated with the organic tracer. The slower accumulation of organic isotope under lighted incubation therefore was not a result of some type of non-biological photochemical reaction:

if it had been, there would have been a difference in filterable isotope levels between the light and dark incubations of the sterilized water system.

Photosynthesis converts inorganic material into cellular and other organic compounds and therefore accounts for the more rapid uptake of inorganic isotope in the light. But light is not known to affect bacterial heterotrophy, so that if the uptake of organic compounds is by bacteria only, the rates of uptake would be similar in both light and dark incubation conditions. The fact that some algae are capable of heterotrophic activities in the absence of light⁵⁻⁷ explains the more rapid uptake of the organic compounds in the dark. That this phenomenon has not been observed by previous ecological investigations using known radiolabelled organic compounds can be explained. A wide variety of dissolved organic compounds occur naturally in aquatic ecosystems, but those compounds (for example, glucose, acetate) used in previous tracer studies exist only in very small quantities⁸, probably as a result of the rapid uptake by bacteria. Certain minimum levels of specific organic compounds are, however, required to induce specific microbial active-transport mechanisms into operation⁹. Natural levels of the previously used tracer compounds may not be sufficient to induce naturally the transport mechanisms of algae into operation and, therefore, when placed in the presence of the tracer compounds, the algae are not physiologically prepared to take them up.

The validity of using sulphur as an indicator of heterotrophy and the possible interference by contaminant $^{35}\text{SO}_4^{2-}$ in the recognition of labelled organic compounds must be considered. Organic sulphur occurs in nature at a very constant sulphur : carbon ratio of close to 1 : 500 (ref. 10) and in aerobic conditions the aquatic sulphur cycle closely parallels the carbon cycle². Aerobic conditions were present in the BOD bottles during these studies, and because of the nature of the organic tracer these results are likely to contain less bias than studies which use only one or two known radiolabelled compounds. Contamination of the organic isotope by inorganic isotope, if it did occur, was of little consequence because maximum uptake of sulphate in a given lake was at least 100 times slower than the observed uptake of organic material (Table 1).

Table 1 Uptake Rates (%/h) of Organic and Inorganic Isotope Sulphur by Plankton in Three Lakes of Different Productivity

Type of lake system	Incubation conditions			
	Inorganic sulphate	Organic sulphur	Light	Dark
Oligotrophic	0.0056	0.0020	0.40	0.91
Mesotrophic	0.0069	0.0018	0.72	1.10
Eutrophic	0.0272	0.0028	1.08	2.15

Although valid comparisons cannot be made between lake systems, two important conclusions can be drawn by examining the activities within each system (Table 1). First, heterotrophic (dark) processes are important in the total uptake of inorganic materials by plankton; second, phytoplankton may conduct up to 50% of the heterotrophic activity that occurs in the limnetic zone of a lake during the dark. If the daily summer photoperiod is 16 h of light and 8 h of dark, then heterotrophic processes may account for up to 30% of the daily sulphate assimilation in an oligotrophic system and up to 10% in a eutrophic system. Similarly, phytoplankton may conduct up to 25% of the daily heterotrophic activity that occurs in the limnetic zone of a lake.

Carbon uptake in light and dark incubation conditions has been shown on numerous occasions, but there has been little quantitative discussion of heterotrophic activities other than the uptake of specific organic compounds. Heterotrophy seems to be an important part of the metabolic activities of plankton, especially in oligotrophic systems, and it is probably an important process in the general metabolic activities of aquatic ecosystems as a whole. There is obviously a great need for

better comprehension of both the qualitative and quantitative activities of microbes in aquatic ecosystems.

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On Vértesszöllös Man

This communication was stimulated by a recent comment by Wolpoff¹. Results of my researches²⁻⁴ on the human fossils recovered from the Mindelian site at Vértesszöllös in Hungary may be summarized as follows. Vértesszöllös man is related by origin to forms such as *Pithecanthropus*, *Sinanthropus*, *Atlanthropus*; he represents an evolved variety of this group ("archanthropic man"), and is probably ancestral to some early progressive forms of Pleistocene man, best represented by the Swanscombe skull. For sake of identification and priority I named Vértesszöllös man *Homo (erectus seu sapiens) palaeohungaricus*. Some workers^{5,6} consider archanthropic man as a conventional grade in the evolution of *H. sapiens*; others reserve for him a separate palaeospecies, *H. erectus*. Once the clade and grade have been determined, however, the name does not change the phyletic position of the fossil.

When Wolpoff criticized me for "placing the occipital in *Homo sapiens*, Thoma's Paläanthropus grade" this is evidently a misunderstanding on his part. He thinks that my lambda localization is wrong because of the presence of wormian bones in this region. There are, however, no wormian bones in the lambda region of the Vsz. II occipital, only small fractures of tabula externa. The occipital is excessively high. The curvature index of the upper part of the squama is well within the modern range², which in turn overlaps with *Sinanthropus*.

My estimate of cranial capacity^{2,4} (>1,400 resp. >1,350 cm) is based on a regression equation which relates the lambda-opisthion chord to cranial capacity in a sample of eleven primitive Pleistocene skulls. The lambda-opisthion chord ranges here from 75 to 94 mm, and because the Vértesszöllös value is 102 or 100 mm, it was necessary to extrapolate beyond

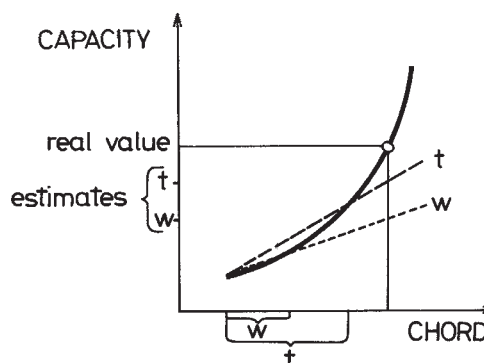


Fig. 1 A possible explanation of the difference between cranial capacity estimates of Wolpoff (*w*) and Thoma (*t*).

known limits. Wolpoff's mean estimate is 1,325 cm³, based on a similar regression equation in a sample of five *Pithecanthropus* and *Sinanthropus* crania. The lambda-opisthion chord ranges here, however, from 75 to 87 mm, making extrapolation more dangerous. The 95% range of Wolpoff's capacity estimate is 980–1,670 cm³, which makes discussion less meaningful. The difference between his mean estimate and that of mine may be explained, however, using a method not considered by Wolpoff (see Fig. 1).

The capacity of a body and a linear measurement of it must be related exponentially. Non-linearity was not demonstrable statistically by eleven crania. The slope of Wolpoff's linear regression (23, 77) which is lower than that of mine (26, 62) taken with the difference in lambda-opisthion range do, however, make likely non-linearity of regression and Wolpoff was thus led to underestimate the cranial capacities. Anatomical reconstruction can be used to estimate capacity, and Gerassimov, who was the best specialist in this field, arrived at an estimate of about 1,400 cm³ (personal communication).

The inferior deciduous canine of *Vsz. I* is, in fact, very similar to the homologous tooth of *Sinanthropus* and also to that of an unpublished Les Eyzies Neanderthal child, kindly shown me by Professor Piveteau.

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Single Channel Theory and Dichotic Listening

It is often assumed in theories of selective attention that dichotic auditory inputs can be considered as separate channels subject to filtering before complete analysis¹. But the fact that highly probable or particularly significant items in the "rejected" message are detected and followed^{2,3} has led to the revision of this "single channel" theory to allow for some degree of analysis of the rejected message^{4,5}. It is also possible, however, that the initial selection takes place on at least two criteria: spatial location of the message (right or left ear) and probability of the item, with high probability of an item overriding spatial location as a determinant of selection.

I have tested this hypothesis by introducing into the unshadowed message an occasional instruction to the subject to tap the table. On the theory that channel selection is determined on a spatial basis, but with simultaneous analysis of rejected messages, the instruction is most likely to be picked up when it coincides with a shadowed item of low probability, because this is known to result in switching to the alternative channel. But if selection is determined on the basis of both probability and spatial origin of the message, the relatively low probability of the instruction to tap will mean that it does not effectively compete with low probability items in the shadowed message and channel selection will be determined on the basis of spatial origin alone.

Pairs of words of high association frequency were used instead of continuous prose⁶. One hundred such word pairs were compiled into two lists of fifty. Six of the second members of the pairs were interchanged at random points in the list, with the constraint that at least two unswitched pairs separated them. The two lists were recorded by the experimenter on two channels of magnetic tape, at a rate of one pair per 1.40 ± 0.04 s. For the tapping experiment, the non-shadowed list presented

Table 1 Sample Items from Left and Right Channels in the Shadowing and Tapping Tasks

Left channel	Right channel	
	Switching	Tapping
you-me	bread-butter	now-arm
dogs-cats	dry-wet	car-jay
top-bottom	shoes-feet	dot-TAP
dark-light	girl-boy	old-wet
rough-queen *	king-smooth *	gay-sad
deep-shallow	here-there	sir-gun
wide-narrow	short-tall	bay-tie
slow-sour *	sweet-fast *	lad-TAP

Asterisks indicate points where the items are switched between channels.

to the right ear was replaced by a list of high-frequency monosyllabic words, containing eight occurrences of the word "tap". Four of these were at points where the left channel contained low-probability second members of the word pairs, and the remaining four at random points, where the coinciding word from the left channel had a high probability. Table 1 shows a schematic diagram of the two experiments.

Eighteen paid subjects from this department were used, who were unaware of the purpose of the experiment. All subjects were given practice with shorter word lists, until they seemed reasonably competent. Eleven subjects performed the shadowing task first, seven the tapping task. Five of the subjects were run with the left and right channels reversed in the shadowing experiment, to check that performance was not influenced by any slight asynchrony between channels. Neither order of task nor channel shadowed influenced the results, which are combined below.

In the shadowing task, subjects were instructed to ignore the message arriving at the right ear, and to repeat as accurately as possible the message to the left ear. In the tapping experiment, the same instructions were given, with the addition that subjects were to tap on the table with a ruler whenever they heard the word "tap" on that channel. It was stressed that their primary task was to shadow.

The probability of an intrusion from the non-shadowed message was much greater at points where an item had low probability (0.32) than with items of high probability (0.07). This difference is significant ($P < 0.001$, Wilcoxon rank test), and replicates one aspect of Treisman's study. The subjects were more likely to process items from the unshadowed message if these had a high probability of occurrence in the context of the shadowed message. In Table 2, results are shown for the tapping study. The probability of tapping on instruction from the right channel was greater when a high probability word appeared on the left channel than when the "tap" coincided with a low probability word in the shadowed message ($P < 0.01$, Wilcoxon rank test); but it is possible to argue that subjects may actually have switched channels in order to tap. Indeed, performance was worse on the shadowing task when subjects tried to tap appropriately. Table 2 shows the probabilities of tapping on instruction from the right ear when the word synchronous with "tap" was correctly shadowed. Whereas many subjects were able correctly to shadow a high probability word and tap ($P = 0.21$), no subject ever shadowed a low probability item and tapped appropriately.

Table 2 Probability of Tapping in Response to Right Channel Instruction according to Probability of Left Channel Word and Whether it was Shadowed

	During high probability word	During low probability word
Shadowed	0.21	0
Missed	0.27	0.30
Total	0.48	0.30

For totals, $P < 0.01$, 1-tailed Wilcoxon rank test.

These results suggest that multiple criteria are used in selection of simultaneously available inputs. On the basis of these criteria, items may be fed through a single channel, as Broadbent originally proposed.

I thank I. Smith for running the subjects, and L. R. Boutler for discussing the results.

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Relationship between Stability and Connectedness of Non-linear Systems

Gardner and Ashby¹ have studied the relation between the probability of stability $p(S)$ and the extent of connectedness ("connectance") C of large linear dynamical systems. They found a very interesting on-off-type (numerical) result: for reasonably large systems (number of components $n \geq 10$) there is a critical value, C_c of C , below which the system is almost certainly stable, but above which it becomes almost certainly unstable.

In this communication we outline the effect of non-linearity on this striking $p(S)$ - C relationship; the general non-linearity of realistic models of physical systems lends particular importance to such an undertaking.

Let the vector function $x(t) = (x_1(t), \dots, x_n(t))$ represent the state of the linear system at time t . The time development of $x(t)$ is given by

$$\dot{x}(t) = Ax(t) \quad (1)$$

that is

$$\dot{x}_i(t) = \sum_{j=1}^n a_{ij}x_j(t), \quad j=1, \dots, n \quad (2)$$

The system is assumed to be autonomous, that is, the a_{ij} s are constants independent of t .

The connectance C of the system (1) is defined as the fraction of off-diagonal elements a_{ij} of A that are non-zero. Thus $0 \leq C \leq 1$. For $C=0$ the system is completely disconnected, and (2) separates into n independent, decoupled equations.

The stability (uniform asymptotic stability) S of the linear system (1) can be related to the eigenvalue distribution of A : if all eigenvalues λ_i satisfy $\text{Re}(\lambda_i) < 0$, then (1) is stable, otherwise it is unstable².

The $p(S)$ - C relationship was discovered in the following manner. (1) A value for n was chosen. (2) The n diagonal elements a_{ii} of A were selected randomly from a uniform distribution in $(-1.0, -0.1)$, that is, the individual x_i were assumed to be intrinsically stable. (3) The x_i s were then coupled by generating $n(n-1)C$ non-zero off-diagonal elements randomly from a uniform distribution in $(-1, 1)$. This completed the construction of A . (4) A was then diagonalized and tested for stability. (5) For a given C steps 2-4 were repeated a large number of times; the fraction of random matrices found stable was then defined as (a measure of) the probability of stability $p(S)$. (6) Steps 1-5 were repeated for different n values.

In general $p(S)$ decreases with increasing C . The larger n , the greater the rate of decrease. The surprising feature is the step-function-like drop of $p(S)$ from 1 to 0 at some critical C value C_c for $n \geq 10$. Gardner and Ashby conjecture that for all

large complex dynamical systems such discontinuous behaviour is to be expected.

The influence of non-linearities on the $p(S)$ - C relation will be analysed in two stages: (a) Effect on S ; (b) effect on C .

(a) Consider the non-linear autonomous system

$$\dot{x}_i = \sum_{j=1}^n a_{ij}x_j + Y_i(x_1, \dots, x_n), \quad i=1, \dots, n \quad (3)$$

where we assume that the Y_i s are convergent power series in x_i , beginning with terms of at least the second degree. Then one has the following theorem of Liapunov³.

(i) If (3) is stable for $Y_i=0$, all i (that is, if (1) is stable), then the equilibrium ($x_i=0$, all i) is asymptotically stable whatever the terms Y_i are. (ii) If (1) is unstable (has at least one λ with $\text{Re}(\lambda) > 0$) the equilibrium is unstable whatever the Y_i are. (iii) If (1) has some roots λ with $\text{Re}(\lambda)=0$ but none with $\text{Re}(\lambda) > 0$, then the terms in Y_i can be chosen such that they have either stability or instability.

This theorem enables us to determine the stability of a class of non-linear systems (3) provided the stability of the precursor linear system (1) is known.

(b) In trying to understand the effect of non-linearity on C one is faced with the problem of defining C unambiguously. (This is not trivial. Even in the linear case one makes the tacit assumption that the x_i s are physically well defined and meaningful components of the interacting system, that is that the coupling of these components does not destroy their physical identity; otherwise, purely mathematical transformations $x_i' = T(x_i)$ can be constructed that would change C arbitrarily.) We ignore this dilemma and assume that the definition of C used for the linear system is also applicable in the non-linear case. We write

$$Y_i(x_1, \dots, x_n) = \sum_{j=1}^n [\sum_{k=1}^n b_{ik}x_k^j]x_j, \quad \text{all } i$$

where the b_{ik} s are constant. Then (3) becomes

$$\dot{x}_i = \sum_{j=1}^n \{a_{ij} + \sum_{k=1}^n b_{ik}x_k^j\}x_j, \quad i=1, \dots, n \quad (4)$$

Assume that the linear constituent of (4) is diagonal and stable (that is, $C=0$, $a_{ij} = -\delta_{ij}a_{ii}$, $a_{ii} > 0$). Clearly, Liapunov's theorem ensures that by different choices of the b_{ik} one can vary C between 0 and 1, without affecting the stability of (4)! Consider now a collection of linear systems (generated via the Gardner-Ashby procedure) which is characterized by a particular ($p(S)$, C) pair. According to our procedure, we can add non-linear terms to each linear system in the collection without affecting its stability either way ((i) and (ii) of Liapunov's theorem; we assume that the probability of (iii) being operative is negligible). At the same time, we can change the C values at will. In particular, the ($p(S)=1$, $C < C_c$) pairs can be changed to ($p(S)=1$, $C_c \leq C \leq 1$).

These results indicate that in the presence of non-linearity the concept of critical connectance may be meaningless in its present form. This conclusion does not deny either the existence or the importance of such a concept; it merely indicates that a more fundamental definition of connectance is needed. Perhaps an approach similar to Kauffman's⁴ will be more successful.

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BOOK REVIEWS

Future Goals

The Place of Value in a World of Facts. (Proceedings held at Stockholm. 14th Nobel Symposium.) Edited by Arne Tiselius and Sam Nilsson. Pp. 496. (Wiley: New York and London, June 1971.) £7.

THE Fourteenth Nobel Symposium was held at the Södergarn Conference Centre just outside Stockholm in September 1969. It took a form different from any of the previous symposia, which have been concerned with one or other of the five Nobel Prize fields—physics, chemistry, medicine-physiology, literature or peace. The fourteenth was described by its president Tiselius as “a kind of ‘cross-cultural’ symposium, covering all five Nobel Prize fields,” and it was the aim of the Foundation “to use its international status and its prestige to do something more to contribute in some way to the welfare of mankind, other than by each year distinguishing a selected few with Nobel Prizes”. The Foundation ventured outside its accustomed judicial role because of its recognition of “a growing awareness among people of all nations that something is wrong with the world, and that there is an urgent need to come together to see what should be done”. The particular emphasis of the symposium, which is expressed in its title, was “based on a feeling that our world today superabounds with facts—and still we are unable to use all this knowledge in an optimal way to relieve mankind of its sufferings . . . the question is, however, what do we want?”

In accordance with these aims, the symposium devoted little time to discussing the major agents of world instability which have become standard fare at most conferences about the future: nuclear warfare, population, food, pollution and so on. Such talks as there were on the threats and promises of technology were on rather less hackneyed subjects. For example, Lederberg contributes a very fine article on “orthobiosis: the perfection of man”, which he classifies into eugenics, euphenics or euthenics, depending on whether the target for improvement is the DNA of the germ cells, the somatic characteristics of the individual, or the environment. J. R. Pearce discusses in an equally authoritative way the “possibilities and pitfalls in electronic information transfer”, dealing both with mass communication and com-

puterization. C. A. Doxiadis provides an essay on urbanization or “The Future of Human Settlements”, which amounts to a succinct exposition of the classificatory and descriptive schemes which he has worked out under the title “Ekistics”, a body of thought and information which he would like us to recognize as a science, but which probably still needs the infusion of more identifiable causal principles before actually earning that name.

Another technological theme is treated in a short but very stimulating paper by Harrison-Brown on “Resource Needs and Demands”. In this he emphasizes a factor the importance of which is often overlooked, namely time. He argues that, contrary to the opinions of some pessimists, man’s survival is not fundamentally threatened by an exhaustion of natural resources: “it can be shown that man could, if need be, live comfortably off ordinary rocks”, but he makes the important point that it is difficult, if not impossible, to see how the present poor nations of the world could be brought up to the standards at present enjoyed in the rich countries in the quantities of refined and processed resources at their disposal. “If by some magic the *per capita* inventory of metals in the world as a whole were brought up to the average level of the ten richest nations, all of the present mines and factories in the world would have to operate for more than 60 years just to produce the capital, assuming no losses. If we were to assume a world population of 10 billion persons (which I suspect is conservative) and a *per capita* steel inventory of 20 tons (in comparison with the present American value of 10 tons), some 200 billion tons of iron would have to be extracted from the Earth. At the current rate of extraction, 400 years would be required.”

The theme of the disparity in the standard of living between the rich and the poor countries was indeed one of the major topics at the symposium. There were important discussions of it by some scientists from developing countries, such as Chagas from Brazil, Lambo from Nigeria, Abdus Salam from Pakistan, and Tata from India, and there were other informed discussions from such internationally minded economists and social scientists as Gunnar Myrdall, J. Tinbergen and W. Guth.

Even these more technologically or economically oriented contributions

were less concerned to describe technological situations or to spell out procedures for dealing with them, than to take as their main focus the problem of what goals should be striven for. Clearly enough, the poorer countries are looking for a good life for their citizens, but what kind of a good life? There is obviously a long way for them to go along the directions which have been so successfully pursued in Europe, the United States, Japan and now the Soviet Union, in raising the material and health standards of their populations. Most of the contributors, however, showed little of the complacency that would be required to argue that the present system by which man in the richer countries conducts his life offers an adequate system of values for everyone else to imitate, even if they could find ways of surmounting the difficulties indicated by Harrison-Brown in the quotation above. Perhaps the most radical dissatisfaction with present value systems was voiced by Jacques Monod in the first sentences of the first paper in the symposium: “The traditional concepts which provided the ethical foundation of human society from time immemorial, all consisted of imaginery ontogenies, none of which remains tenable in the face of scientific enquiry”. He goes on . . . “these societies will collapse, unless a radically new foundation for their value system can be defined, accepted and respected”. Although not all speakers were so forthright, most contributors expressed deep concern at the dangers facing civilization, unless it could find some way of rapidly remodelling its value systems and the ethical foundations on which social life is based. Among people who spoke out in this sense there were such deservedly respected names as Ivan Malek, Linus Pauling, E. H. Gombrich, W. H. Auden, Margaret Mead, Carl Lorenz and several others.

The symposium was organized as a forum for discussion. As Tiselius remarked in opening it, “Nobody expects that such a meeting will solve the problems”. The book closes with a summary report, but not with a series of recommendations. There is, however, considerable importance in the fact that a large body of respected and experienced intellectuals from many fields of endeavour were in general agreement that world society is in danger, not only from the many material problems stemming from applications of technology, but also

from immaterial forces generated by the ineffectiveness or inappropriateness of the value systems on which man's social life has been based. Moreover, it was not only the somewhat elderly, if distinguished, symposium participants who were aware of these dangers. During the week of the meeting an active group of students and other young people took part in much of the social life of the symposium and engaged in some discussions. Their opinions are not recorded in this volume. They were certainly not ready to accept uncritically the *ex cathedra* statements of their elders, but in general they seemed to feel quite as strongly as the participants, if not more so, the need for radical rethinking of social ethics and values. Considerations of this kind must, I think, have a central place in any serious discussion of the future evolution of man in the technological world he has made for himself.

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Geomagnetism

Handbuch der Physik/Encyclopedia of Physics. (Herausgegeben von S. Flügge. Band XLIX/3. Geophysik III, Teil III.) Nenderherausgeber: K. Rawer. Pp. vii + 537. (Springer: Berlin and New York, 1971.) 150.40 DM; \$45.80.

GEOMAGNETISM, so simple in its basic concept, is yet so diverse that the spectrum of geomagnetic variations in time and space reflects changes of intensity of solar wave and particle radiations, physical changes in the Earth's high atmosphere, the constitution of the Earth's mantle; and dynamical changes within its core. Chapman and Bartels, in their two-volume work published in 1940, were able to embrace the subject comprehensively. The expansion of geomagnetism in the past thirty years—perhaps most notably in instrumentation, in the light thrown by palaeomagnetism on continental drift, and in the impetus provided by rocket and satellite measurements—has made it certain that no two authors can now do justice to the breadth and depth of existing knowledge of the subject.

The volume under review, one of several devoted to the subject in this encyclopaedia, is the work of fourteen authors, writing individually or jointly on nine topics. There are two main (unrelated) themes, geomagnetic disturbance and methods of geomagnetic observation. T. Nagata and N. Fukushima make an outstanding contribution of 130 pages on "Morphology of Magnetic Disturbance"—a concise statement of basic physical principles followed by description and explanation of the various forms of disturbance and their relationship with associated

phenomena. There follows, in 70 pages, a brief historical review by E. N. Parker and V. C. A. Ferraro of past attempts to provide a theory of the worldwide magnetic storm and associated aurora, and an outline of the modern (incomplete) theory of the phenomena. The contributions on magnetic disturbance are completed by an article, in German, by M. Siebert on the various measures of magnetic activity, including their statistical properties and the physical significance of these properties.

There are two contrasting articles on observatory measurements of the absolute value of the Earth's magnetic field and the recording of time variations of the field: one, by V. Laursen and J. Olsen, on "classical" observatory methods, the other, by H. Schmidt and V. Auster (in German), on various modern methods of measurement and on the digitalizing of time variations.

Two articles are devoted to airborne magnetometers, the first, by P. H. Serson and K. Whitham, on three-component magnetometers, the second, by J. R. Balsley, on the fluxgate magnetometer but with considerable emphasis also on operational procedures. Finally, on instrumentation, P. J. Hood writes on the geophysical applications (on the ground, in the air, and by satellite) of nuclear and optical absorption magnetometers.

Balsley's contribution suffers from having been written as far back as 1956. This surprising fact is mainly the result of two circumstances: first, the decision of J. Bartels, who was then the designated editor, to delay publication of the volume till the results of the International Geophysical Year could be assessed; second, by the further delay caused by the death of Dr Bartels.

It is fortunate for the volume as a whole that the remaining articles have been updated, or written anew, in one or other of the years between 1966 and 1969.

The last of the contributions to this volume again reflects, one feels, editorial problems. Written, in French, by R. Gendrin, it consists of a description and theoretical discussion of the events (such as "whistlers") which result from the passage of electromagnetic waves through the magnetosphere but would seem more appropriate to have been included in volume 49/4, which is specifically devoted to the magnetosphere.

D. H. MCINTOSH

Transplantation Genetics

Immunogenetics of the H-2 System. Edited by A. Lengerova and M. Vojtiskova. (Proceedings of Symposium held in Liblice near Prague on Septem-

ber 1-3, 1970.) Pp. xii + 360. (S. Karger: Basle, New York and London, 1971.) Sw. francs 110; £12.10; \$26.40.

IMMUNOLOGY is a rapidly growing branch of biological science. Its chroniclers are numerous and its broad-sheets many. Some aspects of the subject have acquired their own technology and language. Just such a one is immunogenetics, particularly that involved in study of the H-2 locus in the mouse. This locus was discovered by the late Peter Gorer who first demonstrated that it had a major influence on transplantation as well as on red cell agglutination. Much of our understanding of the genetic determination of transplantation reactions derives from its investigation. The arrangement of the genes at the H-2 locus, their manifold effects, the nature of their products and their relationship to nearby genes formed the principal topics at the symposium of which this book is a report.

The conference itself is said by those who attended it to have been a great success. The report, however, must be viewed with more caution. The conference took place in September 1970, and to the specialist much of the information it contains is already sadly dated. There are no introductory chapters and little by way of summary. The discussions given at the end of each section are stylized and not easy to enter into. A few realities such as "I agree with Olga . . ." (p. 270) remain—a little research is required to establish the identity of the lady in question.

All this said, however, the book contains a fascinating collection of papers on the H-2 locus. The locus itself is revealed, like its human counterpart, HL-A, to be internally very complex. Aside from the many alleles known at the H-2 locus it has been shown that crossing over within the H-2 region is possible. There is even evidence for genes within the region (Ss and Slp) which do not seem to be concerned with transplantation.

H-2 is part of the IXth linkage group and within the same group, often closely linked to H-2, are found some of the genes which regulate the capacity to make specific immune responses; genes which control resistance to certain leukaemogenic viruses; and genes which determine the capacity to perform in mixed lymphocyte reactions.

There seems to be agreement that the transplantation antigens themselves are glycoproteins attached to the cell surface but there is some disagreement as to whether the specificity of the antigens resides in the carbohydrate or the protein part of the molecule.

The final session of the conference dealt with intraspecific and interspecific aspects of histocompatibility antigens and it is perhaps in making such com-

parisons that the great value of investigation of the H-2 locus will be seen to lie. It affords an opportunity for the study in detail of a mammalian genetic locus which has a wealth of pleiotropic effects and which should surely in the future give us many clues to the nature of some differentiatational processes. This book on the H-2 locus should be looked into by all who aspire, however vainly, to a broad understanding of immunology. But don't expect it to be easy.

A. J. S. DAVIES

The Umbelliferae

The Biology and Chemistry of the Umbelliferae. Edited by V. H. Heywood. (Supplement to the *Botanical Journal of the Linnean Society*. Vol. 64.) Pp. x+438. (Academic Press: New York and London, December 1971.) £8.50; \$26.

FROM time to time, when faced with one of the apparently ever increasing number of symposium volumes, reviewers are wont rhetorically to question the value of such publications. Even the best symposia owe much to the sense of communal presence at lectures and discussions, not to mention the relaxed exchange of ideas with old and new colleagues outside the lecture halls. All of this is lacking from the bare publication of a series of papers the actual information content of which is usually already available in print elsewhere. The not so successful conferences, where successive lectures fail to gel into any sort of accumulative theme, are even more cruelly exposed when published months later as a symposium volume.

Happily, most of these strictures certainly do not apply to the present volume which records the proceedings of a symposium on the biology and chemistry of the Umbelliferae held at the University of Reading in 1970. The key to its success lies in the taxonomic unity and broad sweep of its subject matter. The contents include a historical review of the classification of the Umbelliferae, analyses of the family as it is represented in the New World, Old World and New Zealand, and two accounts of the presumed evolutionary relationships of the family, one based largely on morphological, anatomical and embryological evidence, and the other on chemical affinities; other contributions are surveys of data on pollen grains, inflorescence structure, comparative anatomy, stomata, fruit structure, cytology, floral biology, and the results of phytochemical studies on acetylenic, flavenoid, coumarin compounds and protein-serological work.

Clearly, the scope is most impressive, and the content is in general equal to the coverage since more than a third of

the contributors are leading specialists in umbellifer taxonomy, for example, Cerceau Larrival, Constance, Dawson, Mathias, Rodríguez, together with many other scarcely less eminent systematists and phytochemists. The organizers of this symposium are to be congratulated on mustering such a team and this new volume will represent an important source book for information on the Umbelliferae.

It would be nice to be able to report that this gathering of umbellifer specialists were able to pool their talents and draft at the end of the symposium an up-to-date consensus view of the classification of the Umbelliferae and their allies. Unfortunately, it seems that only politicians are called on to issue "agreed communiqués" after their international meetings, and this volume simply concludes with a scholarly account of the ethnobotany of the family. To be fair, this is perhaps too much to expect of a symposium which was necessarily more of a stocktaking operation and, as Professor Heywood points out in his editorial preface, the volume is intended to be a source of information and is in no sense a systematic account. Certainly there is information here in quantity, but the reader will have to do his own work to extract or correlate much of it. Thus, at the family level chemical data seem to be broadly in agreement with the Saxifragaceae – Pittosporaceae – Sapindales – Rubiaceae – Compositae mesh of affinity which has been suggested by anatomical and embryological studies. As some ideas begin to become clear, however, other possibilities begin to emerge. Particularly with the chemical data one is faced with the general systematic problem that as more information becomes available the network of possible evolutionary affinities spreads. Part of the problem, as Rodríguez in his masterly survey of family relationships in this volume suggests, is that certain established or widely suspected affinities are "tested" by phytochemists while other less advertised possibilities are not. Another aspect is that in the comparison of biosynthetic pathways or molecular structures we are dealing with a level of chemical evolution for which our critical judgments, in terms of assessing significant relationships, are barely mature.

The real value of this excellent volume is that by juxtaposing information available and drawing attention to where information is lacking, it should facilitate further progress in our knowledge of the Umbelliferae. Although the idea of holding a systematic conference devoted to a single family is not novel (the meetings held on the Gesneriaceae and Commelinaceae at the Royal Botanic Garden, Edinburgh, during the tenth international botanical congress

are notable precedents), such single family symposia have been fairly uncommon. The success of this Umbelliferae venture suggests that the format could well be repeated to advantage.

P. E. GIBBS

Semiconductors

Electronic Processes in Non-crystalline Materials. By N. F. Mott and E. A. Davis. Pp. xiii+437. (Clarendon: Oxford; Oxford University: London, December 1971.) £7.50.

THIS important work is the first treatise on the electrical and optical properties of liquid and glassy materials such as liquid metals and semiconductors, amorphous germanium, silicon, selenium and tellurium, and the chalcogenide glasses. The properties reviewed include d.c. and a.c. conductivity, thermoelectric power, Hall effect, and optical phenomena such as absorption and photoconduction. There are also chapters on impurity band conduction, phonons and polarons, and the metal-non-metal transition, which play a significant part in many of the systems being studied.

It is impossible to summarize the extraordinary variety of observation here reported in detail. On almost every page, experimental data of great complexity are recorded—data which are not always consistent with those on the previous page, for this is a notoriously treacherous field for the incautious experimenter. It is quite obvious that much of the ground will have to be gone over again, much more carefully, to establish firm results independent of the accidents of impure or imperfectly defined samples.

To have attempted a survey of this mass of data without a theoretical framework would have produced a mere catalogue, without rhyme or reason. The authors' decision to organize their book about a specific theory was clearly correct, for it sharpens the reader's perception of the problem of interpretation at every stage. A graph of, say, conductivity against temperature is meaningless in itself: our scientific interest is only aroused when we are told that this exemplifies a theoretical formula.

The basis of their theory is explained by the authors in the preface. "The keys to our present understanding have been the principle of Ioffe and Regel that the mean free path cannot be less than the distance between the atoms, and the concept of localization introduced by Anderson. . . ." As they go on to say: "In a sense, our book is written around these two theories. We have built a theoretical edifice on them, and since mathematical rigour is anything but easy in this subject we have not

hesitated to guess at the approximate solution of problems that at present are unsolved. . . . We have chosen the experimental material, too, with a view to comparing it with our theory and our conjectures".

One is bound to ask, therefore, whether a strong case has been made for this theory as a basic interpretation—however rough and uncertain in detail—of the observed phenomena. Is this how it really is? I must admit that I remain somewhat sceptical.

On the one hand, it is not at all obvious that the data themselves fully support the model. In almost every case, agreement between theory and experiment requires that some ill-defined quantity should take some value that has not been checked independently, or it is necessary to introduce one or another of several competing mechanisms that "may" also be acting. Nowhere in the book can I find a clear and simple situation where careful observation and explicit calculation come into close coincidence and where other explanations can be excluded. To achieve conviction by this route, the authors must not only demonstrate that the observations are not inconsistent with a plausibly modified version of their basic model, but they must also show that all competing theories are definitely falsified by the facts. Until that is done, we must remain agnostics, and treat all hypotheses with equal suspicion.

On the other hand, the authors do not demonstrate that their formulae are necessary consequences of the chemical and physical structure of the materials being studied. The Anderson localization theorem, for example, has been proved only for variations of potential in a tight-bound electron band: this proof has not yet been extended to structurally disordered systems such as glassy semiconductors. Again, the "Kubo-Greenwood Formula" (quoted without even a reference to Kubo's original derivation) is notoriously difficult to handle: qualitative dimensional manipulations can prove nothing without a solid backing of tough algebra on exact models. The authors refer in passing to a number of papers which attempt to discuss these points of principle, but cannot expect us to accept their model as necessarily true until they have dealt specifically with genuine objections at the fundamental level.

We must be grateful for the immense labour that the authors have put into collecting and collating these observations, and for the insight and courage of their interpretations and speculations. Nevertheless, on the theoretical side also, this subject is by no means a closed chapter of physics.

JOHN ZIMAN

Aquatic Biomass

Methods for the Estimation of Production of Aquatic Animals. Edited by G. G. Winberg. Translated from the Russian by Anne Duncan. Pp. xii+175. (Academic Press: New York and London, September 1971.) £3; \$9.

THIS useful book not only serves as a guide to methods for estimation of secondary production, but summarizes a considerable body of results. It is a translation of part of a handbook developed for use in the Soviet part of the International Biological Programme. Although it refers to western literature, it will be especially valuable as a review of work in the Soviet Union, much of it done under the leadership of the author.

The first part contains a brief review of methods for measuring and expressing biomass and its components, and discusses the effect of environmental conditions, especially temperature, on growth, development and fecundity. The bulk of the book is devoted to a review of various systems for calculating productivity, based on reproduction and growth. The discussion is divided between two chapters, one for species without continuous reproduction and one for the more difficult case of species with continuous reproduction. Many of the different systems of equations that have been developed by various investigators are merely different arrangements of different symbols representing the same functions, and Winberg points out a number of such identities.

The main approach is to calculate the mass of material that is added to a population during an interval of time, and sum the increments to give the annual total. The emphasis in the IBP view of production is on the tangible increment to the biomass of a population during a period of time. Thus missing from this book is any treatment of energy budgets with accounting of metabolic maintenance costs of populations and the like. The book provides an excellent summary of thinking and work on aquatic secondary production in the USSR. Those who have been bothered by the apparent redundancy of $P = P \cdot B \times B$ will find the most detailed explanation available.

There are relatively few ambiguities or errors; on page 124, the shortcut method of determining egg duration attributed to Tonolli is mis-described. In actual use each ovigerous female is kept isolated, and the production of new eggs is easily detected, so the duration of development is not likely to be overestimated. Furthermore, some of us feel that the reproductive rate of animals is clearly related to food, among other influential factors (page 64). Some of the methods discussed are

in a primitive state of development, and some of them will probably not survive, for example, the use of the generation time for the doubling time.

The translation by Anne Duncan is excellent. The bibliography gives translations of Russian titles and cites sources of English translations of the articles.

This book should not be confused with IPB Handbook No. 17, edited by Edmondson and Winberg. That book covers a wider field including methods of collecting, of statistical analysis and of measurement of such basic processes as feeding, assimilation and respiration. The final chapter of No. 17, compiled by Winberg, reviews methods of calculation, covering much the same material as the book under review, but more briefly and without as much documentation by examples.

W. T. EDMONDSON

Microbes of the Soil

Soil Micro-organisms. By T. R. G. Gray and S. T. Williams. Pp. viii+240 + 11 plates. (Oliver and Boyd: Edinburgh, August 1971.) £3.

THIS book is a well balanced text on soil microorganisms in relation to their environment and hence a good contemporary treatment of soil microbiology.

An interesting feature is the brief treatment given, where appropriate, to some physiological or biochemical process or theory relevant to the subject matter. Examples are the discussion of the regulating mechanism of enzyme production and the principle of the chemostat in relation to microbial ecology. This is not overdone and emphasizes the links between soil and general microbiology.

The book is written in a flowing, very readable style, is thorough, well illustrated, and almost free from misprints. It properly fulfils the purpose of the series in providing an authoritative up-to-date survey of its subject and is to be recommended as a university text. One mis-statement maintains the propagation of the old error that fungi are strict aerobes. For the more advanced reader the book is well worth having not only as a refresher course but for the statements expressed so that they challenge one to think. Why should "fungi and actinomycetes use a greater percentage of metabolized carbon than do bacteria", and how is it that "colonizers of freshly fallen leaves are generally rather uncommon species"—when 25,500 kg of tree litter falls on each hectare of forest? These, and others, are accepted propositions in soil microbiology. Should they be?

H. T. TRIBE

CORRESPONDENCE

Help with Protein

SIR,—“How to help with protein” (*Nature*, 236, 1; 1972) is an excellent review of the protein problem, but it misses the important point, that perhaps protein deficiency is not the most serious problem after all, even for young children. The United Nations document “Feeding the expanding world population: international action to avert the impending protein crisis” (*UN Publication E. 68, xiii, 2*) appeared just at a time when many workers in the field of nutrition were beginning to question the almost exclusive concentration on protein which had characterized the previous decade. It is not accepted by all that kwashiorkor can be equated simply with protein deficiency; some believe that it represents a failure of adaptation to a generally poor diet, others that the precipitating factor is an infection. When energy rather than protein is the limiting factor in the diet, the end result is marasmus, which is less dramatic than kwashiorkor, and has therefore received less attention. Broadly speaking, kwashiorkor may perhaps be regarded as a disease of rural communities, marasmus of urban ones. With rapidly increasing urbanization in most developing countries, marasmus is becoming an ever more serious problem. Since it tends to occur at an earlier age than kwashiorkor, the permanent effects on development are more likely.

Your article was stimulated by the *Eighth Report of the Joint Expert Committee on Nutrition of FAO and WHO*, which it describes as disappointing. Certainly the report does not give a clear picture of the problem, because there are too many gaps in our knowledge. The report poses two questions: first, can one diagnose the nature and extent of the dietary deficiency from the clinical pattern protein-calorie malnutrition? Second, how can one assess the prevalence and type of protein-calorie malnutrition in any community? The answer to these two questions is essential for the proper planning of preventive measures. It may well turn out that concentration on the “protein gap”, and on the practical difficulties of what you describe as artificially fortified foods is leading us down a blind alley. It is necessary to keep an open mind.

In fact your article does this admirably in its closing paragraphs. “It follows that the mere provision of protein supplements is not sufficient and that developing countries . . . have no way of meeting the social need which confronts

them except by developing the kinds of child care services which can ensure that suitable foods are available at the critical times. . . . Such programmes . . . will have to deal not merely with protein supplements but with the health care of children in the round”. These words deserve to be quoted again and again because, as you rightly point out, it is useless to treat these problems in isolation.

Yours faithfully,

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Rothschild

SIR,—Winding up for the Government in the House of Lords debate on the Rothschild Report on February 29, Earl Jellicoe said (about the Research Councils vis-à-vis executive departments) that he saw “no reason why there should be a confrontation . . . between opposing forces” and that he thought he could predict with some confidence that we shall follow “the evolutionary path which I think many noble Lords have suggested we should tread on this particular matter”.

What evolutionary path can we tread? The best answer is to replace the blunderbuss of Table IV of Lord Rothschild's Report by inter-governmental machinery in which the research councils would be included.

All parties are agreed on the transfer to executive departments of the financial responsibility for most of the tactical science done by the research councils, once the departments have fully competent Chief Scientist organizations. This could in due course transfer funds of £7 or £8 million. The work involved can be fairly easily defined and identified, and the transfer would not raise problems of accountability or threaten the stability or integrity of the councils.

To go beyond transfers of this magnitude involves arguments about strategic and basic research where different but equally valid definitions give different answers. Moreover, the transfer to executive departments of funds with which the research councils finance basic and strategic research cannot but cut across their ability to discharge their primary responsibilities, which are to the Secretary of State for Education and Science.

In any case, the objective of organizations is that executive departments

should be able effectively to influence the programmes of the councils so as to take account of national responsibilities of the departments in the same or related fields of research. In that case, there are two steps on which all the parties are agreed—the creation of powerful Chief Scientists' organizations within departments and the granting of full council membership to the heads of those organizations. These two developments would go a long way to securing the main objective. True, such an arrangement would entail partnership and joint involvement, as opposed to the method of confrontation which would almost certainly result from giving departments the power of the purse, and good will and good sense by all would be required, but that is not a weakness—rather a necessity for any organization to run well.

But what will happen when a department and a research council disagree about a programme or series of programmes, or where a department considers that too much money is being allocated to the research councils as a whole? At present there is no machinery for catering for this kind of situation. The advice of the Council for Scientific Policy goes straight to the Secretary of State for Education and Science, who deals directly with the Treasury. There is no place, and no point in time, at which departments can quarrel with the amount of money being allocated to the DES vote. This is the one remaining gap in the machinery which must be closed.

So should not departments have the right to take major disagreements on research council programmes or financial allocations direct to the CSP? The CSP should make its own judgment of such a dispute in framing its advice to the Secretary of State, making clear in every case what the respective arguments are and why they have decided the one way or the other. It is to ensure that the CSP should not automatically rubber stamp the views of the research councils in such a situation that this institution, while wishing the CSP to remain advisory, has supported the proposal of the Dainton Working Party that the Chief Scientists of Departments should also be members of the CSP. The Cabinet would, of course, be the ultimate referee.

It is, surely, along these lines that a solution must evolve. Instead of arbitrarily wresting from the councils a large part of the finance they have already secured to discharge their responsibilities to the Secretary of State for Education and Science, machinery must be created

to provide a framework within which departments and research councils can work in partnership while allowing for the occasional major difference of view to be discussed and determined at every level.

Yours faithfully,

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Publication Speed

SIR,—I am pleased to see that Mr St Aubyn has, in responding to my letter, taken the opportunity of giving us some good advice on how to get our papers published (*Nature*, 235, 447; 1972). I am unsure about the sin that I and a number of other contributors have committed. I had always believed that while it was quite true that I could not eat my cake and have it I could certainly have it and eat it. I agree, I think, with the substance of every point that he makes in his letter, but disagree considerably about their importance.

Journals must, of course, have stringent refereeing procedures, but there is no need for this to take longer than, say, three months. Drs Rose-Innes and Lipman (*Physics Bulletin*; September 1971, 527–8) tell us that a good PhD thesis (longer than Mr St Aubyn's already excessive 8,000 words) takes about the pre-electrification time from Manchester to London to read and assess. All working scientists read research reports and critically evaluate them for their own purposes in very short periods of time. Three months seems a maximum for these decisions.

I would agree that there are likely to be some cases where the subject matter of submissions is so technical that a generalist editor cannot know whether it suits his journal or not. This surely cannot be the case on other than an insignificant number of occasions; surely it is this matter of relevance which is above all the responsibility of the editor.

Finally, I'm sure that the majority of readers of my original letter will know that I never suggested that authors should adopt the practice of multiple publication. I merely stated that in the case in which many journals are clearly so inefficient the editors should not be too surprised if

this device was used as a tactic to ensure speedy and full publication.

Yours faithfully,

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Knuckle Walking

SIR,—Russell Tuttle and Benjamin B. Beck state (*Nature*, 236, 34; 1972): "Man, too, often rests his flexed fingers in facultative knuckle-walking postures, as may be witnessed in certain public speakers and football linesmen". But surely the distinctive hand flexure of linesmen is due to the fact that they must hold a stop-watch in their non-flag-waving hand, the face of which must be easily visible? Is it different in Chicago? Does this affect the evolutionary inferences?

Yours faithfully,

E. N. TIRATSOO

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Obituary

Dr John Dennis Carthy

JOHN CARTHY, who died on March 13, was a man of many talents and much of the success and enjoyment which he achieved was brought about by the ease with which he combined them. His main scientific interests were in animal behaviour, but his knowledge and lively enthusiasm soon led him into activities where his ability to understand and communicate this work became appreciated by many. Those who met him found a warm and friendly personality which quickly made him a most approachable and rewarding colleague.

John Carthy was born in Plymouth in 1923 and educated at Bedford School and Christ's College, Cambridge, where he took the Natural Sciences Tripos in 1947. For the next three years he was engaged in postgraduate work under Professor W. H. Thorpe on problems of the orientation of ants and their use of odour trails. He was awarded the degree of PhD in 1950 and left Cambridge to join the academic staff of Queen Mary College, University of London, where he remained for the next seventeen years. During this period he extended his behavioural interests to include research on sensory physiology and cell ultrastructure. The

research published on these problems included investigations on the electrophysiology of the eye of the "whillig" beetle, the fine structure of the sensory pegs of scorpion pectines and field observations on the behaviour of a variety of arthropods. A number of successful postgraduates were trained under him at this time on topics as diverse as the significance of olfaction and vibration in termites to the social behaviour of the Red Avadaat. He was also involved in the establishment of a successful biological electron microscopy unit at Queen Mary College.

In 1952 John Carthy went on an expedition to the Azores and wrote a number of papers on the insect fauna of the isles of Pico and Fayal. An excellent entomologist and field worker, he blended these interests with his behavioural work so that he was eminently suited to be appointed in 1967 as the first Scientific Director of the Field Studies Council. In this capacity he travelled around the country a great deal, and soon became a familiar figure at the field stations, where he encouraged a whole range of research interests among the staff. He was particularly proud of the Oil Pollution Research Unit which was set up at Orierton and shared the concern for the environ-

ment expressed in the joint publication on the *Biological Effects of Oil Pollution on Littoral Communities*.

To many John Carthy will be remembered as a scientist who carried his subject to a wide audience by his radio and television appearances. Others will have read his many books, of which *Animal Navigation*, *An Introduction to the Behaviour of Invertebrates*, *The Behaviour of Arthropods*, *Animal Behaviour* and *The Study of Behaviour* are perhaps the best known for the clear and enjoyable style which appeared in these and later publications and which attracted a whole generation of biologists. The extent of the impact which these books had can be judged from the fact that they were translated into Spanish, Italian, Dutch, Swedish and German. These popular achievements were, however, based on the solid foundation of an understanding of scientific research and its implications and it was for the same reason that he was in heavy demand by the scientific community. He served as Secretary to the Society for Experimental Biology and the Association for the Study of Animal Behaviour and was a section secretary for the British Association for the Advancement of Science, of which he was also a Council member. He was

on the Council of the Royal Entomological Society and a Vice President in 1962. His services were similarly used in education where he was chief examiner of a number of organizations, a committee member of the Education Subcommittee of the Zoological Society and on the Biological Education Committee of the Royal Society and Institute of Biology. He was made a Fellow of the Institute of Biology in 1967.

As a scientist he travelled widely, and in 1962 lectured at the Royal Ontario Museum. He visited Warsaw in 1964 through the hospitality of the Polish Academy of Sciences, and was invited to the University of Louvain in 1967.

These achievements, however, represented only some of the facets of the man, for more than anything else John Carthy realized the relationships of science to other studies. His interests in art, drama and literature sprang from the same source as the critical faculties, understanding, humour and patience which he gave so freely to his colleagues in science.

Professor Louis Joel Mordell

PROFESSOR L. J. MORDELL, the acknowledged doyen of British number theory, died on March 12, 1972, at the age of 84.

Louis Mordell was born an American citizen in Philadelphia in 1888. Attracted to mathematics by books in a second-hand store, he noted that many of the problems were taken from Cambridge Scholarship and Tripos papers, so, in 1906, he crossed the Atlantic alone to sit the scholarship examination of the St John's group and was awarded a major scholarship. He took Part I of the Mathematical Tripos in 1909, the last year of the published order of merit, and was third after P. J. Daniell and E. H. Neville. He remained in Cambridge until 1912, when he was awarded the second Smith's Prize. After periods at Birkbeck College, the Ministry of Munitions and Manchester College of Technology, he became Fielden Professor of Mathematics in the University of Manchester in 1923 and was elected to the Royal Society in 1924. A capable and conscientious head of department, he built up a strong school. In 1945 he left Manchester to succeed G. H. Hardy in the Sadleirian Chair in Cambridge. After his retirement in 1953 he retained his base in Cambridge but travelled much and was for several years a visiting professor in the USA and Canada. He retained his passionate interest in mathematics and mathematicians unabated and continued to publish and to lecture on his work right to the end.

Mordell's most famous individual

achievement is his "finite basis theorem". This deals with rational points on algebraic curves (that is, with points (x,y) with rational coordinates which satisfy the equation $f(x,y)=0$ of the curve). Algebraic curves are classified geometrically by a non-negative integer, the genus. For curves of genus zero the properties of the rational points were completely elucidated at the turn of the century. For curves of genus greater than one, there is still no theory of any generality, though much work has been motivated by Mordell's conjecture that there are at most finitely many rational points. It is curves of genus one which have the greatest interest today since there is an extensive theory as well as plausible but recalcitrant conjectures: and it is here that Mordell's theorem is central. Poincaré had already shown that the rational points on a curve of genus one have a natural group structure. Mordell showed in 1922 that, as conjectured by Poincaré, this group always has a finite basis. Weil generalized this to abelian varieties.

Throughout his life Mordell was fascinated by Diophantine equations of all kinds (that is, the solution of equations in rationals, or, alternatively, in integers) and he displayed a quite remarkable virtuosity. His only substantial book, written when he was already an octogenarian, is a valuable account of this fascinating but little systematized area. A particular favourite was the determination of all the integral points (x,y) on the curve $y^2 = x^3 + k$, where $k \neq 0$ is a given integer. Special cases had been considered already by Fermat and others but the equation is sometimes known as "Mordell's equation". The Smith's Prize essay of 1912 developed several lines of attack and determined all the solutions for many values of k . At the time Mordell believed it to be possible that there could be infinitely many solutions for certain k , but he subsequently realized that his work, together with a then little known result of Axel Thue showed that the number was always finite. The finiteness proof was, however, non-constructive; that is, there was the logical possibility that one could never be sure whether a given set of solutions was complete or not, and so it remained a challenging problem to find complete sets of solutions. In 1947 Mordell chose $y^2 = x^3 + k$ as the subject for his Cambridge Inaugural Lecture, which was published by the University Press under the title *A Chapter in the Theory of Numbers*. In a sense this chapter was closed by Alan Baker who, as a special case of his general theory, has given an effective procedure for finding all the solutions for given k , albeit one that requires multilength computer programs for its practical implementation.

Rather similar to Diophantine equa-

tions is the theory of polynomial congruences, which occupied Mordell at several stages of his career. The problem here is to study or estimate the number of solutions of a congruence $f(x_1, \dots, x_n) \equiv 0 \pmod{p}$, where f is a polynomial with integer coefficients and p in the most interesting cases is a prime. Mordell brought his habitual ingenuity and dexterity to bear. His earlier results were superseded by deep estimates of Hasse and Weil but he returned to the subject later and showed that his techniques were still valuable in a more general context. Similar remarks apply to Mordell's contributions to the estimation of trigonometric sums.

Mordell was an expert in modular forms and functions. In 1917 he proved one of the famous conjectures of Ramanujan about his "tau-function", the coefficients of a certain modular form. Later Hecke was to show that the property in question follows from those of the "Hecke operator" and is a special case of a general phenomenon. Mordell used modular theory to investigate certain peculiar recurrence relations between the class numbers of imaginary quadratic forms which had been discovered by Kronecker, Liouville and others and showed that they were best understood as resulting from equating coefficients in identities between modular forms. He also used modular theory to investigate the number of representation of integers as sums of squares, particularly the more difficult case where the numbers of squares is odd.

Another of Mordell's major interests was the Geometry of Numbers. Not merely did he contribute himself but he sparked off the renaissance of the subject in the 1940's and 1950's associated in addition with the names of Mahler, Davenport and Rogers. Perhaps his most notable result was the determination of the lattice constant of the region $|f(x,y)| < 1$, where f is a binary cubic form.

No brief account can do justice to the variety and interest of Mordell's work. His achievements and influence rank him with the masters of the theory of numbers.

Announcements

International Meetings

May 1-3, **Breeding of Endangered Species**, St Helier (The Secretary, Jersey Wildlife Preservation Trust (ESI), Les Augrès Manor, Trinity, Jersey, Channel Islands).

May 8, **Movement for Survival**, London (Michael Schwab, Department of Nutrition, Queen Elizabeth College, London W8 7AH).

May 9, **Crop Protection**, Ghent (Symposium over Fytopharmacie en Fytiatrie, Faculteit van de Landbouwwetenschappen, Coupure 553, B-9000 Ghent, Belgium).

May 11, **Europe: British Food Technology on the Threshold**, London (Institute of Food Science and Technology, 41 Queen's Gate, London SW7).

May 17-19, **Isoelectric Focusing and Isotachophoresis**, New York (Associate Executive Director, The New York Academy of Sciences, 2 East 63rd Street, New York, NY 10021, USA).

June 5-6, **Biological Degradation of Lignin**, Quebec (Dr Marcel Jean, Département de Biochimie, Faculté des Sciences, Université Laval, Quebec 10, PQ, Canada).

Reports and Publications

not included in the Monthly Books Supplement

Great Britain and Ireland

The Hannah Research Institute for Studies Relating to the Production and Utilization of Milk—Report for 1971. (University of Glasgow.) Pp. 48. (Ayr, Scotland: The Hannah Research Institute, 1972.) [142]

Greenwich Time Report, Royal Greenwich Observatory. Time and Latitude Service, 1971, January-March. Pp. 187-205. (London: Science Research Council, 1972.) [142]

Textiles, Vol. 1, No. 1, February 1972. Pp. 1-28. (Didsbury, Manchester: Shirley Institute, 1972.) [142]

Philosophical Transactions of the Royal Society of London. B: Biological Sciences. Vol. 263, No. 851 (10 February 1972): The Skull and Skeleton of *Eogyrinus attheyi* Watson (Amphibia: Labyrinthodontia). By A. L. Panchen. Pp. 279-326+plates 24 and 25. £1.40; \$3.65. Vol. 263, No. 852 (10 February 1972): Insect Faunas of Late Devonian and Flandrian Age from Church Stretton, Shropshire. By P. J. Osborne. Pp. 327-367. £1.10; \$2.86. (London: The Royal Society, 1972.) [142]

Science Policy, Vol. 1, No. 1, January/February 1972. Published bi-monthly. (London: Science Policy Foundation, 1972. Published in collaboration with OECD, Paris.) [152]

The Macaulay Institute for Soil Research. Collected Papers, Volume IX, 1967-1969. Edited by A. M. B. Geddes and R. Noble. Pp. 29+reprints Nos. 1-87. (Craigiebuckler, Aberdeen: The Macaulay Institute for Soil Research, 1971.) [152]

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PEP. Broadsheet 1972: The Organization of Voluntary Service: a Study of Domiciliary Visiting of the Elderly by Volunteers. By Barbara Shenfield and Isobel Allen. Pp. iv+183. (London: PEP, 1972.) £2. [162]

A Memorandum by the Council of the Royal Society of the Consultative Document (CommD 4814) 'A Framework for Government Research and Development'. Pp. 60. (London: The Royal Society, 1972.) [172]

Discovery Reports. Vol. XXV, pp. 199-358: Sperm Whales Off Durban. By Ray Gambell. £10 net; \$27.50. Vol. XXXVI, pp. 1-94: Life Cycle of Antarctic Krill in Relation to Ice and Water Conditions. By N. A. Mackintosh. £7 net; \$19.50. (London and New York: Cambridge University Press, 1972. Issued by the National Institute of Oceanography.) [172]

University of Birmingham. Annual Reports and Accounts for 1971. Pp. 48. (Birmingham: The University, 1972.) [172]

Joint Organization for Solar Observations. Annual Report for 1970. Pp. 61. (Cambridge: The University, The Observatories, 1972.) [172]

The World Land Use Survey. Occasional Papers No. 11: Welenkomi: A Socio-economic and Nutritional Survey of a Rural Community in the Central Highlands of Ethiopia. By Mesfin Wolde-Mariam et al. Pp. 67 (31 plates). (Berkhamsted, Herts.: Geographical Publications, Ltd., 1971.) £2.50. [172]

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Minority Rights Group. Report No. 8: The Africans' Predicament in Rhodesia. By G. C. Grant. Pp. 31. (London: Minority Rights Group, 1972.) 30p. [172]

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Water Resources Act 1963. Eighth Annual Report of the Water Resources Board for the year ending 30th September 1971. Pp. vii+94. (London: HMSO, 1972.) 73p net. [182]

Other Countries

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Australia: Commonwealth Scientific and Industrial Research Organization. Annual Report of the Wheat Research Unit, 1970/1971. Pp. 26. (Ryde, NSW: CSIRO, 1972.) [172]

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Canada: Department of Energy, Mines and Resources. Bulletin 209: Redescription of *Marrella splendens* (Trilobitoidea) from the Burgess Shale, Middle Cambrian, British Columbia. Pp. 31+26 plates. \$3. Paper 71-37: Radiocarbon Geochronology of Southern British Columbia. By R. J. Fulton. Pp. iv+28. \$1.50. Geological Map 1306A: Tanquary Fiord, District of Franklin. (Ottawa: Information Canada, 1971.) [182]

Office de la Recherche Scientifique et Technique Outre-Mer Initiatives—Documentations Techniques, No. 18: Les Phlébotomes (*Phlebotominae*) et les Maladies Qu'ils Transmettent. Par A. V. Dolmatova et N. A. Demina. Traduction Française de A. Kobylansky. Texte revu par E. Abonnenc et J. Rageau. Pp. 168. (Paris et Bondy: ORSTOM, 1971.) 35 francs. [222]

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